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# RUTHERFORD: LIFE AND WORK TO THE YEAR 1919, WITH PERSONAL REMINISCENCES OF THE MANCHESTER PERIOD

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*The first Rutherford Memorial Lecture of the Physical Society; delivered 6 November 1942*

IN March 1942 the Council of the Physical Society decided that the first two Rutherford Memorial Lectures should be "reminiscent and personal". This decision accords with a general precept stated by Rutherford himself in a letter* to Rayleigh in 1936:—"I am sure that we all ought to .... give some of our recollections of those past and gone before they are lost for good." In writing this he had in mind a younger generation that shocked him by appearing to "have the impression that Science only started in their time", and his argument loses nothing of its force from the fact that a veneration for the great figures of the present and the immediate past was not always a conspicuous feature of his private conversation.

Even in a country which is shamefully neglectful of the history of science, and in a world where one country has for years striven mainly to distort it, Rutherford will not easily be forgotten; he has made too deep and wide a mark for that, and there is too much of his work in both the foundations and the superstructure of modern physics. From this point of view, Rutherford stands less than most men in need of any formal memorial, but a Memorial Lecture can still serve a very good purpose by helping to give, especially for the rising generations of physicists, a picture of the man and of the way in which his work was done. I say deliberately, the way in which the work was done, rather than the work itself; for the latter stands, where everyone may see it, in the literature of physics.

The only discernible slackening in the progress of Rutherford's scientific work, from young manhood to the end of his life, was that imposed by the war of 1914–19; at the end of this period he moved from Manchester to Cambridge. It is therefore by an entirely natural—indeed, loosely speaking, by the only possible—dichotomy that the period allotted to this lecture is that which ends with Rutherford's departure from Manchester.

It is obviously impossible, either in the space of an hour's lecture or in the somewhat larger compass allowed to the lecture in its published form, to do justice to Rutherford's life and contributions to science during this period.

* Quoted in Feather, *Lord Rutherford*, p. 57.

Any attempt to do this would inevitably lead to the compilation of a mere catalogue of names and facts. Furthermore, even if it were possible, the existing biographies* and obituary notices make it unnecessary. I shall mention, therefore, only such few salient points as are needed to give an outline to the picture; my main aim will be to give some impression of Rutherford in Manchester and of the atmosphere of endeavour and achievement which he maintained in his laboratory. In this way I may be able to add something new to the much that has already been published. Even if some of these additions are trivial—too trivial, perhaps, to find a place in a serious biography—I hope that they may help towards the construction of a faithful picture of Rutherford in his early prime.

I do not think that I underestimate the difficulty of getting an accurate picture. We have, it is true, a very valuable source of information in Rutherford's own correspondence, a great part of which has, through Lady Rutherford's care and foresight, been preserved and filed. This gives us, expressed in his own vigorous English, much first-hand information about him that would otherwise have been lost. The facts remain, however, that Rutherford's genius was of a most uncommon kind, which will not, I think, ever be completely analysed, and that his outlook was less simple than many people believed it to be.

My own picture of him, which I have to try to convey in this lecture, is bound to take some of its colour from the circumstances in which I first knew him. When I first met him, he was at the height of his powers and I was a raw student, fresh from school and barely half his age; I began to work with him (naturally, in a not very complicated research) two years later. I have stated these facts, much as a junior student might enter in his laboratory note-book, at the beginning of an experiment, the room temperature and barometric pressure, and with the same lack of certainty about their relevance to what follows. It is probable that if I had been at a less impressionable age when I first met him, or more nearly his equal in age or attainments, I should have arrived at my picture of him by a different process, but I do not think it would in the end have been a very different picture. He would possibly have had a very different picture of me, but that at least is quite irrelevant to the present occasion.

## NEW ZEALAND, 1871–1895

Rutherford's paternal grandfather, George Rutherford, a wheelwright, left Scotland in 1842 to settle, with his family, in New Zealand. His son James, born in 1839, married in 1866 Martha Thompson, who had come to New Zealand at the age of twelve, and who for a short time before her marriage worked as a school teacher. Ernest Rutherford, born on 30 August 1871, was the fourth of the twelve children of this marriage. He was born at Brightwater (Spring Grove), not far from Nelson; four years later the family moved to Foxhill, and in 1882 to Havelock, both in the neighbourhood of Nelson. Ernest attended the State primary schools at Foxhill and Havelock, and apparently was very well taught at both schools. In 1886 he won a scholarship which took him to

* A. S. Eve's *Rutherford*, the official biography, gives the life and work, with copious extracts from the correspondence. N. Feather's *Lord Rutherford*, a shorter work, gives a sketch of the life, with an excellent connected and critical summary of the work and a few brief extracts from the correspondence.

Nelson College, and at about the same time his family moved to Pungarehu, in North Island—not very far away—where his father, who had previously worked as a wheelwright, farmer, timber contractor and engineer, now concentrated on large-scale flax production.

Nelson College was evidently a good and progressive school, with an excellent headmaster in W. J. Ford and a fine mathematical master in Dr. W. S. Littlejohn, who gave Rutherford a sound start in mathematics and physics. Rutherford had a very successful three years in Nelson, winning awards not only in his best subject, mathematics, but also in Latin, French, English literature, history, physics and chemistry. It is on record that in June 1888 he was top of the Sixth Form in *all* subjects. In the case of French, which he also later took as a degree subject, I feel sure that his success could only have been due to sheer competence and a powerful determination to succeed; although all his physical writings were crystalline in their clarity, I do not think that he had any real affection for the study of languages. Certainly, when Jean Perrin visited Manchester about 1912 or 1913, Rutherford adopted standard British practice in communicating with him—that is, he spoke in English, rather faster and even rather louder than when addressing a compatriot, with no more concession than an occasional *voici* or *voilà*, *oui* or *non*, to Perrin's almost complete incomprehension of rapid colloquial English.

In 1890 a Junior University Scholarship took Rutherford to Canterbury College, Christchurch, where again he established a record of unbroken academic success. It was here, too, that he met, and formed a deep attachment for, Mary Newton, who became his wife in 1900.

In seniority, Canterbury College was the second institution of university rank to be established in New Zealand. Otago University was founded in 1869, Canterbury College in 1873, and the Colleges at Auckland and Wellington in 1882 and 1897; the University of New Zealand was constituted in 1870. At the time of Rutherford's entry Canterbury College was still small; it had about 150 regular students—Rutherford's name is No. 338 in the college register of students—and 7 professors. Mathematics was taught by Professor C. H. H. Cook, physics and chemistry by Professor A. W. Bickerton, and it is difficult to imagine a more strongly contrasted pair of teachers. Cook was evidently a sound mathematician and an excellent teacher along orthodox lines, with no marked tendency to stray from those lines. On the other hand, Bickerton's outstanding characteristic was a lively and unbounded enthusiasm for experimental science in all its manifestations. Feather * illustrates his widely ranging interests by quoting two of the seven papers down in his name at an annual meeting of the New Zealand Institute (November 1894); they were: "On molecular attraction" and "On chlorine as a cure for consumption". He was most in his element in speculations on such topics as the rejuvenescence of the cosmos; cosmology was in fact his hobby, if that is not too light a term. There is a very revealing passage, relating to the beginnings of his cosmological researches, in his book, *The Birth of Worlds and Systems*, published in 1911.† It runs: ".... although I held South Kensington teaching certificates

* *Lord Rutherford*, p. 24.

† Rutherford wrote a short preface to this book.

in thirteen branches of science, I knew but little of Astronomy, so I had to study the subject". In all his work he was uninhibited by any lack, conscious or unconscious, of essential preliminary technical knowledge, and to the end of his very long life he remained eager to learn and to advance the cause of science. It is doubtful whether Rutherford's scientific curiosity ever needed sharpening from without, but it is certain that if he had needed a stimulus he would have found it in Bickerton's laboratory.

Bickerton was a remarkable man, and, I think, by all but the harshest academic standards, a great man. Rutherford used to speak of him with affection, and in an obituary notice after his death in 1929 he paid tribute to the part he had played, by his enthusiasm and his encouragement of original investigations, "of whatever kind", in promoting science in New Zealand.

It was in Bickerton's laboratory, under the most primitive—not to say sordid—conditions, that Rutherford did his first research work. This was on the magnetization of iron in rapidly alternating fields, and it was followed by the invention of an ingenious and highly sensitive magnetic detector of Hertzian waves. These contributions, which would have been remarkable enough as the first performance of a young researcher under any conditions, must be classed very highly indeed in the light of what we know of the facilities and assistance available to Rutherford at the time.

It should be remembered that New Zealand was then a very young country. It had attracted a very fine type of settler, and it is to the everlasting credit of the early settlers that they did so much to develop a sound educational system, in the face of financial shortage and the difficulty of serving a widely scattered population. When George Rutherford entered New Zealand, the number of white settlers was no more than about 2000; there was no rapid increase either in the numbers or the prosperity of the community, and the first two University Colleges were in fact founded when the total white population was a bare quarter of a million, and predominantly agricultural. It so happened that the year in which Rutherford was born coincided with the beginning of a seven-year period of great prosperity throughout the country, and by the end of the period State schools had, by the Education Act of 1877, been made free, secular and compulsory. It is clear from Rutherford's family history that his parents would in any circumstances have been prepared to make all sacrifices that were possible and necessary to secure the best available education for their children; nevertheless, it is an arresting thought that if Rutherford had chanced to be born a very few years earlier, it is highly improbable that he could have got the early training necessary to direct him into physics. No one who knew him can doubt that, whatever his early training had been, he would have become a great man—but he would have been a different kind of great man, and in all probability the whole face of modern physics would be very different from what it is now.

## CAMBRIDGE, 1895–1898

There was, too, an element of chance in the award of the 1851 Exhibition Science Scholarship which enabled him to come to England in 1895. The examiners' first choice was a chemist, J. S. Maclaurin of Auckland, who was a year senior to Rutherford. Maclaurin, for family reasons, did not take up the

award, and the scholarship passed to Rutherford. It was, of course, no chance that took him to Cambridge, and to J. J. Thomson at the Cavendish Laboratory. He arrived there just before the beginning of the Michaelmas Term, beating J. S. Townsend by a short head for the distinction of being the first "advanced student" to enrol under Thomson for the degree of B.A. by research, which was just being instituted in Cambridge for workers from other universities. This passage, from a letter* written to Mary Newton on 3 October 1895, is well worth quoting:—"I am very glad I came to Cambridge. I admire Thomson quite as much as I thought I would, which is saying a good deal."

Here he continued to work on Hertzian waves with his magnetic detector, and he had notable success in detecting waves at—for the time—very great distances from their source. He soon, however (Easter, 1896), dropped this to work with Thomson on the new subject of ions in gases, building up in a remarkably short time a very complete theory of gaseous conduction and determining experimentally many of the characteristic properties of the ions. It is interesting to note that Rutherford's entry into his life's work was determined by two events, both of which were entirely unpredictable at the time of his arrival in Cambridge—namely, the discovery of x rays by Röntgen in November 1895 and of radioactivity by Becquerel in February 1896. Both these discoveries were largely accidental, and the second of them was made during the following of a false trail from the first—though there was, of course, nothing that can be described as even remotely accidental in either Röntgen's or Becquerel's approach to, or subsequent development of, his discovery.

It is obvious that Thomson and Rutherford's pioneer work on gaseous ionization depended entirely on this timely discovery of suitable ionizing agents. Thomson's earlier work had indeed prepared some of the way, but further advance had to await the discovery of a relatively powerful, convenient and steady means of ionizing gases at ordinary pressures. The Röntgen tube was the first of all such devices—though it is hardly likely that the adjectives "convenient", "powerful" and "steady" would be among the first to occur to a modern radiologist who was asked to work with a tube of the kind available in 1896, fed by an induction coil.

It was almost inevitable that after working on the ions produced by x rays, Rutherford should go on to examine those produced by the Becquerel rays, and the passage thence to the study of the radiations themselves was almost equally natural. There was, therefore, once again an element of chance—an accident of time and place—in Rutherford's entry into the field of radioactivity. But for this he might well have continued to give his attention to electromagnetic waves, and it is quite likely that he would have risen to fame mainly as a pioneer of wireless communication. From 1898 onwards, however, there was never any element of chance in his choice of research; he made and pursued his own line as few other men have done, and it would scarcely be an exaggeration to say that his subsequent achievement would have been little less imposing or significant if, in the whole period under review, he had been completely oblivious to everything that went on outside his own mind and his own laboratory.

We may with advantage pause here to recall the state of atomic physics at

* Eve, *Rutherford*, p. 17.

the time of Rutherford's first Cambridge period. It was a time of almost unparalleled activity. Within a very few weeks of the two discoveries already named, there was a remarkable development in electrical theory. Lorentz, the greatest of the pioneers of modern electron theory, quantitatively explained the newly discovered Zeeman splitting of spectral lines in magnetic fields as a consequence of radiation by charged particles. In particular, he showed that the specific charge of the particles had to be assumed to be about a thousand times that of the hydrogen ion in electrolysis. Negative electrons, possessing this property, were isolated for the first time by J. J. Thomson in 1897, and evidence was rapidly accumulated in support of the view that they were constituents of all ordinary matter. This may well be taken as the starting-point of modern electrical and atomic theory.

Soon afterwards, Townsend, Thomson and others measured the electronic charge, $e$, and Townsend's brilliant work on the diffusion of gaseous ions established its identity with the charge carried by a monovalent electrolytic ion. At the same time C. T. R. Wilson was engaged on his investigations of cloudy condensation on gaseous ions, which played an important part in the early determinations of $e$, and which led some years later to the invention of possibly the most searching tool ever put into the hands of atomic physicists. Thus the stage was well set for the part which Rutherford was destined to play upon it.

In the summer of 1898 he left Cambridge to take the Macdonald Research Professorship of Physics at McGill University, Montreal. The decision to leave Cambridge could not have been an easy one to make, for his work was progressing rapidly, and its success had been recognized in 1897 by the award of the valuable Coutts Trotter Studentship of Trinity College. It seems certain that he was influenced by his desire for a position sufficiently assured to justify an early marriage, and the conviction that, as an "advanced student", he would be prevented by local prejudice from getting such a position in the Cambridge of 1898. It is equally certain that this belief was due to no act of Thomson's, for J.J. had always championed the cause of the research men from other universities, had always treated Rutherford with great personal kindness, and had given him every encouragement in his work. Rutherford, moreover, must have known that of the first three great holders of the Cavendish Chair only one (Rayleigh) had escaped the contamination of undergraduate life at another institution, for Maxwell had studied at Edinburgh University and Thomson at the Owens College, Manchester. However, it is not likely that he was altogether wrong in thinking that lesser men than Thomson had resented the influx of "advanced students", and it is, moreover, clear that at this stage he had not yet realized that he was himself of the stuff of which Cavendish Professors are made. Thomson clearly had a very high opinion of him, but Rutherford's excessively modest view of his own powers is clearly enough expressed in a letter* written to Mary Newton (August 1898) on the Montreal appointment. "It sounds rather comic to myself", he said, "to have to supervise the research of other men, but I hope I will get along all right." Anyone with the least knowledge of what he did in McGill will find it much more than "rather comic" that he should have harboured such doubts; but,

* Eve, *Rutherford*, p. 55.

in fact, this diffidence was far less uncharacteristic of Rutherford than many people imagine. Years afterwards, when he had made the Macdonald Laboratory world-famous, he showed himself surprised and almost inordinately gratified that foreigners like Hahn and Godlewski should think it worth while to go there to work with him.

There is no doubt that Rutherford could, if he had so chosen, have obtained a more lucrative post than the Macdonald Professorship. He was never much interested in money, and the excellence of the research equipment at McGill was evidently in his eyes a sufficient compensation for the smallness of the salary offered (£500). As he said himself in a letter * to his mother: "The salaries are small compared with the endowment of the laboratories and the enormous money spent on them, but that is chiefly due to the fact that the money has been advanced by Macdonald, a millionaire, who made his money in tobacco, and he lives on £250, so he reckons a professor should live on £500. However, £500 is not so bad, and as the physical laboratory is the best of its kind in the world, I cannot complain."

## MONTREAL, 1898–1907

He arrived in Montreal in September 1898, and was soon hard at work. As a newcomer, and as a very young professor with practically no teaching experience, he might have had many difficulties to surmount, but these were largely swept out of his way by his senior colleague in the physics department Professor John Cox. Cox is perhaps best known in England (though less well known than he ought to be) by his delightful book on *Mechanics*,† an elementary work in which the subject is treated historically. He was not a great research physicist, but he was a scholar and a perfect colleague. He helped Rutherford's research work by shielding him as far as possible from administrative and elementary teaching duties, and he gave him valuable, timely and reasoned support when some of his colleagues, unable to appreciate the new advances, were inclined to regard them as unsound, gravely revolutionary, and a menace to the reputation of the university. Cox's understanding and wholehearted support must have been worth a great deal to Rutherford in those early days.

The outstanding advance associated with the McGill period was the development in 1902–3 of the transformation or disintegration theory of radioactivity, published as a result of the joint work of Rutherford and Soddy. The disintegration theory was a staggeringly bold hypothesis to come from two comparatively young workers, one in his first independent command, the other barely past the threshold of his scientific career. It must be difficult, if not impossible, for the young physicist or chemist of the present day to realize how extremely bold it was, and how unacceptable to the atomists of the time; it triumphed because, and only because, no other theory was capable of explaining the then known facts of radioactivity.

Among the facts for which an explanation had to be found were: the evolution by radioactive bodies of large amounts of energy, often with no measurable diminution, over long periods of time, in the amount of active matter; the

* Eve, *Rutherford*, p. 57 ; Feather, *Lord Rutherford*, pp. 61, 62.

† Cambridge University Press, 1904—now out of print.

production and gradual disappearance of the emanations; and the complicated behaviour of the active deposits—originally regarded for a time as evidence of an excited or induced radioactivity. Here, in particular, was a mass of experimental data which might well have seemed incomprehensible; the activity of the deposits varied in a most complicated way with the time, its course depending not only on the length of the preliminary exposure to the emanation, but also on the type of electroscope used for the measurement—$\alpha$-, $\beta$-, or $\gamma$-ray. In some cases an $\alpha$-ray electroscope would show a rapid initial fall in the activity of the deposit, while a $\gamma$-ray electroscope would show a less rapid initial *rise*, the curves having only one thing in common, namely, that all could be represented by the sum of a number of terms, in which each term varied exponentially with the time. The clue was found in the particularly simple rise and decay curves obtained after suitable chemical separations of members of the uranium and thorium series. Starting from this, Rutherford and Soddy developed the theory of successive transformations, which explained all the known facts and, what is even more important, proved an infallible and indispensable guide for further advances. Even if the theory had ultimately proved to be untenable, its formulation and application would rank as major intellectual achievements in the history of physics. This is a point which needs to be stressed, for the younger generation is more likely to be familiar with the ordered simplicity of the radioactive series, as we now know them, than with the chaotic state which preceded the transformation theory, and may therefore underestimate both the magnitude and the difficulty of the advance which was made.

By the time Rutherford left Montreal for Manchester, in 1907, the theory had been established beyond all reasonable doubt, and many of the details of the sequences of transformations had been elucidated. Rutherford had, moreover, established, in his own mind at least, the nature of the alpha particles and had made rough measurements of their velocities and specific charge. He had also measured, with Barnes, a number of heating effects. He had received many academic honours, his fame was securely established, and he had made of his laboratory what its founder had always hoped it would become, a permanent centre of research in Montreal. Higher education in Canada owes much to Rutherford—and this indebtedness has been generously and repeatedly acknowledged. On the other side of the balance sheet, McGill gave to Rutherford the material needs for an immensely fruitful nine years' work, and he was always ready to point out that the benefits of his association with Canada had been by no means one-sided.

## MANCHESTER, 1907–1919

Rutherford's election to the Manchester professorship was largely due to the foresight and initiative of Arthur Schuster, who had marked him down some time before as the successor he would wish to see in the Langworthy chair of physics. Schuster was, for a physicist, a wealthy man; moreover, like so many of his race, he knew not only how to be generous, but also to be so with rare discrimination. This he showed when, on retiring at a relatively early age to make way for Rutherford, he enormously increased the strength and improved the balance of the physics department by persuading the

University Council to institute a readership in mathematical physics, and by paying the reader's stipend out of his own pocket for some years. Schuster was a fine physicist, with unusually wide interests, and he had built a laboratory that was, for its time, very well designed and equipped, and that could produce adequate supplies of liquid air. It was, naturally, less well equipped than McGill for work in radioactivity, but Rutherford was never a stickler for luxury in apparatus—in fact, if he had a fault as an experimenter, it was that he was too modest in his demands for material equipment—and he was to be found setting up an emanation electroscope in his new laboratory on 6 June 1907, less than three weeks after he had left Montreal.*

Rutherford evidently liked Manchester from the first, and it is a great tribute to his personality that Manchester took to him so warmly—for he was always very plain-spoken, and Lancashire men, almost as markedly as their Yorkshire neighbours, combine a great pride in their own plain-speaking with an even greater intolerance of plain-speaking in others. Nevertheless, Rutherford soon became, and always remained, one of the most popular figures in the University. He had, it is true, as a newcomer is apt to have, a few clashes with vested interests, but as he got much the better of these clashes he bore no malice afterwards—or, more accurately, no more malice than sufficed to provide occasional mild amusement to his friends.

His chief handicap in the first weeks was a shortage of radioactive material, which threatened seriously to curtail his opportunities for work. The Vienna Academy arranged almost at once to lend radium for the joint use of Rutherford and of Sir William Ramsay, but it soon became clear that a sharing arrangement between two strong individualists, each of them fully convinced that the interests of science could be best served by his own virtual monopolization of the active material, was unworkable. Rutherford might doubtless have found the arrangement just tolerable if he himself had been given charge of the radium, but he could not easily resign himself to the existing position, which was that Ramsay had possession of the whole supply in his own laboratory at University College, London. Fortunately, the Vienna Academy made another generous loan of 450 mgm. of radium bromide direct to Rutherford for his own use, and this supply, sometimes by diplomacy, sometimes by swift evasive action, and finally by purchase, he was able to keep in his laboratories to the end of his days.

The Vienna radium was received in January 1908. It was put into solution, and simple but extremely efficient arrangements were made for drawing off and purifying the emanation. After this, although at times there was fierce competition in the laboratory for active sources, there was no really chronic shortage of material. Rutherford was exceptionally fortunate in having in the laboratory at this time one of the most distinguished of the pioneer radiochemists, his old friend, Professor B. B. Boltwood, of Yale, whom he persuaded to come to Manchester for the session of 1907–8, and who shared with him the responsibility of handling the radium.

Rutherford was fortunate also in the men he found in the Manchester laboratory, most of whom gave up the work they were doing and turned to radio-

* Feather, *Lord Rutherford*, p. 115.

activity.* He was perhaps most fortunate in finding Hans Geiger, a young graduate of Erlangen, who had already done research under Schuster, and William Kay, a young mechanic who soon afterwards became head steward of the laboratories. Kay, besides being a first-rate mechanic, always appeared to have an instinctive knowledge of the anatomy and pathology of any piece of apparatus, however complicated and unfamiliar it might be. In his younger days he was a fine athlete, well known on the amateur running track, but in a very few years the laboratory appeared to become his sole hobby; he used to put in very long hours there, working a week that would have quite rightly been condemned by any trade union. Every physicist knows the importance of the part played by the technical staff of a laboratory (see, for example, Sir J. J. Thomson's *Recollections and Reflections*, p. 115); Kay must be ranked high amongst the world's greatest laboratory stewards and lecture assistants, and no account of Rutherford's Manchester period can be complete without some reference to him. Much of the credit for the smooth running of the laboratory is due to his efficiency, and to his evenness of temper, which survived even the assaults of successive generations of heterogeneous and polyglot groups of research workers.

Geiger was a well-trained and capable all-round physicist, with a considerable command of experimental technique and an almost gluttonous appetite for work. His work with Rutherford on the electrical counting of alpha particles —an amazing technical feat in those days—was completed within a few months, and the resulting paper, together with that on the total charge carried by the particles, was read to the Royal Society in June 1908. Geiger was later appointed Research Assistant and made responsible for training research students in experimental radioactivity. The training course was substantially that described in Makower and Geiger's *Practical Measurements in Radioactivity*, published in 1912.

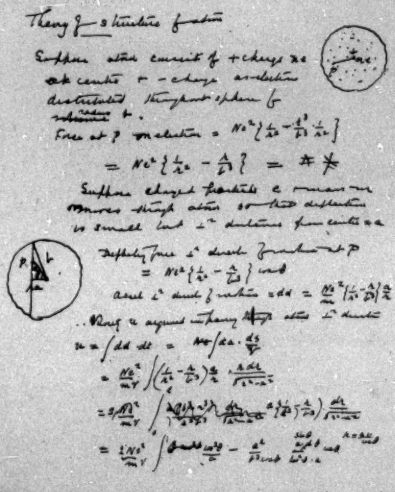
Rutherford was now attracting experienced workers to Manchester from many other universities, and he had even learnt to accept this as normal. He was so full of ideas for work that he needed every man he could get, and he trained as many as possible of his own honours students for research. It was customary for such students to take a short course of measurements under Geiger as part of their second-year laboratory work, and to begin practically full-time research on a selected problem in their third year at the University. The main degree examination in physics took place at the end of the second year, and a year later there was a paper on modern physics, and more especially radioactivity, for which students were expected to prepare mainly by the reading of current periodical literature. This scheme of undergraduate training, as I experienced it, had the grave disadvantage of leaving wide gaps in our knowledge of some important sections of classical physics—and this is putting it euphemistically, for the word gap normally connotes at least something more or less solid in which a gap can exist.† On the other hand, under Rutherford the scheme

* Spectroscopic research was, however, always kept alive in the laboratory—one result of this being that after the publication of Bohr's theory of spectra, E. J. Evans was able to obtain spectroscopic confirmation of Bohr's attribution of the Pickering and Fowler lines to helium.

† In later years, Rutherford considerably stiffened the lecture courses, and himself took a greater part in them.

did give to all students the chance of seeing physics as a living and growing science; the best students did, as they always will, get hold of their physics somehow, and the system produced a steady stream of men who could, under Rutherford's and Geiger's supervision, play useful parts in radioactive investigations.

Rutherford's most fruitful contribution to general atomic physics arose directly out of the results of an experimental problem which had been given to Ernest Marsden, one of the first of the Manchester trainees. This was the work on the large-angle scattering of alpha particles, carried out mainly by Geiger and Marsden in collaboration. Approximately in Rutherford's words,* spoken long afterwards, "I agreed with Geiger that young Marsden, whom he had been training in radioactive methods, ought to begin a research. Why



MS. page of Rutherford's rough notes: early steps in the theory of the nuclear atom.

not let him see if any α-particles can be scattered through a large angle? I did not believe that they would be .... It was quite the most incredible event that has ever happened to me in my life. It was almost as incredible as if you had fired a 15-inch shell at a piece of tissue-paper and it came back and hit you." Thus originated Rutherford's nuclear theory of the atom, which he worked out in all essential details in 1911, and which changed the whole face of modern physics. Bohr's application of quantum principles to the motions of the electrons of the nuclear atom followed in 1913, when Bohr was back in Denmark after spending some months in Rutherford's laboratory. In the same year Moseley's beautiful work on x-ray spectra still further clarified the picture by establishing the precise significance of the concept of *atomic number*. Rutherford's first

* Abridged from a report by Mr. J. A. Ratcliffe of Rutherford's last public lecture, in *Background to Modern Science*, p. 68 (1938).

public announcement of the nuclear theory had been made in March 1911, and the detailed theory appeared in the *Philosophical Magazine* in June. Bohr's three papers on "The constitution of atoms and molecules" and Moseley's first paper on "The high-frequency spectra of the elements", all communicated by Rutherford, appeared in the same journal between July and December 1913. In the meantime, full confirmation of the nuclear model had been obtained by Geiger and Marsden in a very complete investigation of α-particle scattering.

The years 1911–13 may be regarded as marking the end of the first great phase of radioactive investigation, for the outstanding problem of that period was completely solved by Moseley's identification of the atomic number of an element with the number of natural units of positive charge in its nucleus. The radioactive displacement law, which had been built up on purely chemical evidence by Soddy, Fajans, Fleck, Russell and others, could thenceforward be seen as a necessary consequence of nuclear theory. From this time onwards, interest centred on nuclear physics rather than on classical radioactivity, as it might now be termed.

Rutherford's next attack was upon the problems arising out of the collisions of alpha particles with light nuclei—the earlier work having dealt with collisions with nuclei heavy enough to be sensibly motionless throughout a collision. On the theoretical side the problem was attacked by C. G. Darwin, who was Schuster Reader at the time; the first experiments were made by Rutherford and J. M. Nuttall and by Marsden, who had become Research Assistant in 1912, on Geiger's departure to Charlottenburg. Rutherford was also turning his attention to problems of β- and γ-ray emission, and with E. N. da C. Andrade, who succeeded Moseley as John Harling Fellow in 1913, he made the first determination of γ-ray wave-lengths by an ingenious modification of the Bragg method of reflection from crystals.

Research on nuclear physics in Manchester was, however, slowed down by the opening of the first phase of the world war. Rutherford's research school was almost completely dispersed, and most of its younger members were soon afterwards engaged, on one side or the other, in the European conflict. Fortunately for the routine work of the department, Rutherford was able to retain Evans, and Bohr came as Schuster Reader in place of Darwin, who was in France. For the next four years much of Rutherford's time was taken up by work for the Government; it is characteristic of him that he contrived in this period to carry through successfully yet another epoch-making research. In experiments which he began in 1917 and ended early in 1919, generally with Kay alone to help in setting up apparatus and counting scintillations, he succeeded in showing that the nitrogen nucleus could be disrupted by the impact of a swift α-particle, with emission of a hydrogen nucleus. This, the first detected example of "artificial transmutation", was the last work he published from Manchester; its further development belongs to the second Cambridge period.

* * * * *

When I, as a young student, first met Rutherford, in October 1908, he was at the height of his powers—as he had indeed been for many years, and as he remained to the end of his life. Of my predecessors whom he had met a year

earlier, at the beginning of his first session in Manchester, he wrote to Boltwood*: "I find the students here regard a full professor as little short of Lord God Almighty. It is quite refreshing after the critical attitude of Canadian students. It is always a good thing to feel you are appreciated." I can testify that the same reverence was felt for Rutherford by my generation and succeeding generations of Manchester students—though he was quite wrong in believing, if he really did believe, that all full professors were similarly honoured.

Officially, students specializing in physics had no lectures from the professor in the first year, but in practice we used to attend his elementary lectures as regularly as we could, partly because they were really illuminating, whatever their topic, and partly for the beautiful, and often sensational, experiments which Kay prepared for them. It almost seemed at times that the course of the lectures was determined, not by any preconceived plan of Rutherford's, but by Kay's taste in lecture demonstrations; sometimes—not often—in the course of a lecture Rutherford would lower his huge voice to a huge and perfectly audible whisper, and appeal to Kay for information about the experiment he was demonstrating. This in no way detracted from Rutherford's command of his class; Manchester students at that time were far from docile, but he never had trouble, even with the largest classes, largely because of his knack of getting and keeping their interest in what he had to say.

We saw him at his best and most inspiring at the physics colloquium, which met on Friday afternoons. The meetings, at which we were joined by large numbers of chemists and mathematicians who came over to hear Rutherford, were preceded by an enormous tea-party, generally presided over by Lady Rutherford. Rutherford always addressed the first meeting of the session, giving with obvious enjoyment a summary of the main researches carried out in the laboratory during the preceding year. I am fairly certain that this inaugural address was always called "The progress of physics, 1907–1908", or whatever the session might be, and that the choice of topics was always made in the same way—or, as we should have said a little later, by the same selection principle. I am quite certain that in the atmosphere of sustained and justifiable enthusiasm which Rutherford created at those meetings, no young Manchester student could fail to feel that he was a member of a highly privileged community. I think we all felt that we were living very near the centre of the scientific universe, and maturer consideration has only strengthened my conviction that we were right. The sense of privilege naturally grew stronger in those who went on to do research, even in those of us who were doing relatively humble tasks, and not, like Moseley, things that were going to matter fundamentally in science for years to come.

At this stage it is platitudinous to say that Rutherford was a born researcher, or that he enjoyed, in all senses of the term, an even less common gift for leadership and companionship in research. His gifts are easier to recognize than to analyse, but obviously some analysis is called for in a memorial lecture. I think it wiser not to attempt anything so presumptuous as a formal analysis, but to give a simple and straightforward account of how Rutherford worked, and kept others at work, in Manchester during the years that I knew him.

* 20 October 1907. Eve, *Rutherford*, p. 167.

In the middle years he generally had in the laboratory between fifteen and twenty men engaged in full-time work in radioactivity, and it must be remembered that as a rule a large proportion of these consisted of young workers for whom he had to provide fairly constant supervision as well as the original idea and scheme of attack. The supervision he provided in full measure, in his daily rounds of the laboratory, and he was brimful of ideas—in fact, there was never a time when he had not more problems ready for attack than men available to attack them. He excelled in devising direct and simple methods of attack that could be carried through with a minimum of elaborate or costly apparatus, and at this stage in the history of radioactivity there were many urgent problems requiring only moderately accurate measurements for their solution. His young assistants could therefore have the unusual satisfaction of doing productive work during their apprenticeship, and—a point of some practical importance—Rutherford had the best of reasons for not worrying about the possibility of half-trained men doing irreparable damage to valuable instruments.

Shortage of apparatus for advanced work, especially where high vacua were needed, was undoubtedly a difficulty at times, and I have never been able fully to understand how even Rutherford managed to keep everyone going. No doubt he was able to tap a number of external sources for special apparatus for his major researches, but much of the work of the laboratory must have been financed out of ordinary income. Dr. Norman Smith, a senior member of the Chemistry staff in Manchester in Rutherford's time, and now Registrar of the University, has been kind enough to get for me the details of the annual grants made for apparatus and equipment to the Physics Department during these years. The accounts are slightly complicated by the fact that until 1912, when it became a separate department under the direction of Professor Robert Beattie, Electrotechnics was administered as a sub-department of Physics. Excluding amounts earmarked for electrotechnics, and two special Government grants of £100 each, the average for 1908–1914 was under £420 a year, and for 1915–19 under £100 a year. With these grants Rutherford worked wonders, and there can be few instances in the history of science of comparably rich yields from such modest investments.

Earlier vacuum work in the laboratory was done mainly by preliminary exhaustion with a simple Toepler pump, completed when necessary by charcoal in liquid air. Later, a few Gaede rotary mercury pumps were imported, but even those of us who were lucky enough to get one of these could count only on the very intermittent use of an oil pump for the necessary fore-vacuum; we had to put the largest bottle we could get into the pumping train, in order to reduce to a minimum the number of times we had occasion to "borrow" the Fleuss pump. The general shortage of apparatus obviously called for a good deal of give and take on the part of everyone in the laboratory; on the whole, this did not cause serious trouble, though a few people are still remembered by their greater readiness to take than to give, rather than by the work that they did.

For some years Geiger acted as a watch-dog over the research apparatus, and although he was a jealous guardian his popularity and prestige were high enough to enable him to do this without much friction. Some things he particularly hated to see leaving his charge, and after an interval of thirty years

I can still remember his sorrow on handing over to me a battery of small accumulators that I needed for the electrostatic deflection of alpha particles. This was a battery of 2400 volts, made up of sixty banks each of twenty lead accumulators of a well-known test-tube type. The test-tubes were very fragile, and the whole design of the battery highly vulnerable. Before handing it over, Geiger delivered a little homily: I was never to touch the battery connections while I was standing on the concrete floor; I must always keep a dry wooden board to stand on while making adjustments, and I must always hold one hand firmly behind my back while touching any part of the battery, so that there could be no risk of a circuit being completed through my body. Before I had any chance of expressing surprise at, or gratitude for, his solicitude, he went on, with obviously complete solemnity and singleness of mind: "You see, if you get a bad shock you may kick out before you realize what you are doing, and the Prof would not like it if some of the cells got broken." Geiger, by the way, nearly always referred to Rutherford as "the Prof", while most other people spoke of him as "Papa"—the pronunciation and accenting depending upon whether the speaker was or was not an habitué of the local music-halls. Possibly Geiger thought this appellation disrespectful—if he did, he was wrong.

I have already mentioned Rutherford's daily rounds of his research workers; in these he collected information of any new results, performed prodigies of rapid mental arithmetic in interpreting them, made his suggestions for improvements in apparatus or methods of observation, and discussed difficulties that had arisen. Here, going as he was in quick succession from one problem to another, his apparently intuitive grasp of essentials was remarkable to see. It is true, of course, that most of the problems had originated with him; he had a favourite axiom that "No man should ever have more than one problem given to him"—meaning that after working on one problem a man should know for himself what to do next—but he was far from consistent in honouring this precept. He much too often had another problem waiting for the man before the first was nearly finished. The daily visits to the research men derived much of their stimulating effect from Rutherford's own passionate interest in the course of the work and his obvious impatience to know the result of each experiment; even the laziest worker was bound to be infected with something of his interest and enthusiasm, or at worst to be imbued with a healthy desire to avert his active disapproval. Rutherford himself was human enough to take pleasure in the idea that one of his functions was to "ginger people up", as he usually put it, and he did not altogether dislike the feeling that he was pushing along a part of his team, as well as leading the rest.

I am sure that the laboratory tea-table, situated in the radioactivity training laboratory, was far from being the least important bench in the laboratory. Rutherford provided tea and biscuits every day, and nearly always attended himself, sitting at the table, with the rest of us perched on stools and the neighbouring benches. It was a period of relaxation and general gossip, but the meeting often resolved itself into an informal colloquium, with Rutherford taking rather more than a chairman's part. It was here, too, and in the frequent hospitality enjoyed by the research men in his own house, that Rutherford'

essential friendliness was most apparent. He had in a very high degree that friendly and companionable spirit which is a notable characteristic of people who have spent much of their lives either in New Zealand or Canada. This undoubtedly had its effect in keeping the laboratory working as a more than commonly united family. Rutherford had, moreover, what is not typical either of England or of any Dominion, a curious insensitivity to differences of academic or social standing, within fairly wide limits, and it probably was very good for the morale of a young research student to see himself treated with as much—or as little—respect as an emeritus professor.

Perhaps the greatest single factor in Rutherford's success as a leader was his own obvious and enormous delight in experimentation. In the days of which I am speaking he always seemed to be at his happiest when he was in the laboratory and working with his own hands or designing his own apparatus. I remember once wasting with him the whole of a fine Saturday afternoon in an obviously rather hopeless effort to purify, with a few dregs of liquid air, a very dirty little sample of radon with which we had hoped to work. The attempt ended with a momentary and quite uncharacteristic lapse on Rutherford's part, which resulted in the admission to the sample of a much larger volume of air than we had previously succeeded in extracting—a slip which brought out the very characteristic remark, "Well, it's a good job *I* did that, and not you." I am afraid I felt that the afternoon might have been better spent, but Rutherford's final comment, as he sucked contentedly at his pipe while we cleared up the mess, was: "Robinson, you know, I *am* sorry for the poor fellows that haven't got labs. to work in!"

This is, perhaps, in one respect a misleading example of Rutherford's delight in experimental work; I have not wished to suggest that his pleasure would have survived a sequence of wasted afternoons or that he could have found permanent satisfaction, as Henry Cavendish did, in working in isolation and with no thought of publication. In fact, he was far from indifferent to questions of priority in discovery—he liked "to get there first" and to waste as little time as possible in doing it. I never felt more certain of having pleased him than on the one occasion on which I was lucky enough to get some results far more quickly than he had expected. He happened to say one evening, just before he went home to dinner, that he thought it ought to be possible to get evidence of definite groups of secondary $\beta$-rays from lead or other heavy elements, and that we must try it some time with the magnetic spectrograph. As the apparatus we had used for the primary $\beta$-rays was only partially dismantled, and as there chanced to be a suitable radon source which I could "borrow" for the night, I made a trial run, and was amazed to find that I could get a measurable photograph with an unexpectedly short exposure. As a result, by the time Rutherford returned, about nine o'clock next morning, I was able to show him four or five magnetic spectra obtained with different fields and different thicknesses of lead. I still remember vividly the impatience with which I awaited his arrival, and ever since that time I have been quite certain that I understand exactly the feelings of a fox-terrier as, after killing a rat, he brings it into the house and lays it on the drawing-room carpet as an offering to his domestic gods.

This story is not so pointless as it may sound; what I want to stress is that,

over and above the usual incentives to research, Rutherford's co-workers in Manchester had an unusually powerful stimulus in their leader's enthusiasm and never-failing keenness, and the desire to please him was a far from negligible factor in the output of the laboratory. It lost nothing of its potency from the fact that Rutherford pleased was a vastly more comfortable figure to have in the laboratory than Rutherford displeased; his pleasure and displeasure were obvious enough to the least experienced and most obtuse. Newcomers soon learnt that the sight of Rutherford singing lustily "Onward, Christian soldiers" (recognizable chiefly by the words) as he walked round the corridors was an indication that all was going well. I was interested to learn, many years later, that this apparently quite unconscious habit dated at least from the McGill period.* I do not know if this is true of his other habit of intoning a melancholy dirge (never completely identified, for obvious reasons) when work was not going well, or when he had found someone maltreating a treasured piece of apparatus.

It is perhaps unnecessary to add that Rutherford's attitude to the non-radioactive work in progress in the laboratory was relatively detached, spectroscopic investigations, for instance, being regarded with benevolent, rather than passionate, interest. It is easy for anyone who knew the laboratory to realize how it came about that when Andrade arrived he was at once set to work on a $\gamma$-ray problem, though previously he had been working on single metal crystals and on ionization in flames. Similarly, although Darwin came as a mathematician without having suffered the tedious preliminary training which is usually imposed on young physicists, it surprised no one very much to see him in the laboratory in shirt sleeves, at work on the experimental investigation of a branching radioactive series.

I remarked not long ago that Rutherford was far from indifferent to questions of priority in discovery—he liked to be recognizably first on a new trail. This tendency had probably been accentuated in Canada when, at a time of rapid advance, his distance from Europe caused delays in publication. "I have to keep going, as there are always people on my track", he wrote to his mother from Montreal in January 1902.† As this is a manifestation of a trait which is liable to be reprehended by critics who are more coldly academic in outlook, or less energetic in exploration, than Rutherford was (and it is not difficult to be either), it would be unfair to let my remark stand without amplification. It was not, I think, true that Rutherford was inordinately anxious for recognition, but rather that his attitude was exceptionally frank; it should be remembered, too, that he was uncommonly generous in assigning credit to others and in the matter of joint authorship. He had always done a large share of the work in any paper which bore his name as part author, and there were many papers to which he had contributed the original idea and much help, but which did not bear his name.

I can think of nothing more that I can usefully add to this account of Rutherford as leader of his Manchester research school. The period is fittingly summed up in Rutherford's own words, in a letter written to Geiger many years

* A. Norman Shaw, *Proc. Phys. Soc.* **50**, p. 452 (1938).

† Eve, *Rutherford*, p. 80.

later*: "They were happy days in Manchester and we wrought better than we knew."

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In the nature of things, there can obviously be no young physicist who knew Rutherford as long ago as the Manchester period, and there must be many of the rising generation who have never seen him at any time; it is therefore fitting, if not imperative, that this lecture should contain some attempt at a personal account of the man, as distinct from the physicist, as he was in middle life. To deal first with externals, he was a powerful and impressive figure. He stood very little, if anything, under six feet high, and his large frame, if never overloaded with fat, was in middle age what is tactfully described as well-covered. It has frequently been said that he had the appearance of a typical farmer; I think it would be more accurate to say that he looked like a town-dweller's somewhat idealized conception of a typical prosperous and good-tempered farmer. He certainly had, in spite of the long hours he spent in the laboratory and his study, the fresh skin and clear eye usually associated with an outdoor life. Captain Oswald Birley's portrait, executed for the Royal Society, has admirably captured the essential Rutherford of a later period; although it was painted in 1932, it is also very recognizably the Rutherford of 1919. A good impression of Rutherford in the early Manchester days may be obtained from a well-known photograph which shows him with Geiger in the laboratory. This photograph and the Birley portrait are reproduced in our *Proceedings* for 1938.† The photograph, naturally, shows the face in repose, and though it is technically good, it fails to give the impression of great power that one always received on meeting Rutherford in the flesh.

It is with considerable diffidence that I approach the task of describing Rutherford's personality. It is never easy to be objective about the recently dead, and Rutherford was so full of vitality to the last that it is difficult even now to realize that it is over five years since he died. My task is simple in one respect only, namely, that I know nothing about him that there is any need to suppress or whitewash. If there is ever a revival of the unpleasant practice of making literary capital by caricaturing the less agreeable foibles of the truly great, none of its practitioners will be able to make anything of Rutherford, for he had none of the meaner faults.

I do not suggest that he was a copy-book hero, for this he was not, and I cannot think off-hand of any description (except perhaps that of "intellectual") that would have offended him more deeply. On the whole, he suffered fools far less gladly than is enjoined upon us, but if the folly remained outside certain fairly well-defined limits, Rutherford was as likely as the next man to treat the fool tolerantly, or even kindly. He found it difficult to tolerate pomposity or pretentiousness—or, more accurately, he did not try very hard to tolerate them—and he was very impatient of anyone who discoursed at greater length than he liked—when, for instance, he wanted to take the floor himself, or to go away and do something else. On such occasions he could be, and frequently was, quite ruthless, though after he had thoroughly trounced the offender (and,

* Ibid. p. 358 (2 September 1932).

† *Proc. Phys. Soc.* **50**, facing pp. 455 and 466 (1938).

to do him justice, on occasion when the offender had surprised him by coming back with a well-placed retort) his temper was generally soon restored.

Crimes against apparatus he kept in a special category; for such crimes he had little forgiveness and an uncomfortably long memory. In this respect he was uncommonly like Uncle Pentstemon, who at Mr. Polly's wedding, it may be remembered, could see in the bride's elder sister only the young fiend who, many years before, had mucked up his mushroom bed with her clumsy great feet. Similarly, although Rutherford was, at any rate after the first few seconds, eminently reasonable about any accident that could fairly be ascribed to ill-luck, his view of a co-worker was apt to be coloured for many years by recollections of a bad case of carelessness.

I have already mentioned his outstanding friendliness, which was a very real characteristic, with no trace of self-consciousness or seeking after popularity, and no taint of the cultivated toothy smile which sometimes simulates friendship; in fact his oldest friend, if he arrived inopportunely, was more likely to be received with a grunt than a smile. Few men can have made more friends, or lost fewer, than he did, and it was delightful to see his pleasure in the company of old friends like Elliot Smith or Boltwood. He liked rejoicing with his friends, and was quick in sympathy with their major misfortunes, though at times he might hugely enjoy the contemplation of their minor mishaps. Like most men who keep their hair, but are not perfectly confident that they will always do so, he took a keen and not quite seemly interest in the signs of approaching baldness in his contemporaries and his juniors, but in most other respects he gave the lie to La Rochefoucauld's best-known maxim.

His mood in the laboratory was generally one of good humour, and frequently boisterous good humour, but he suffered from occasional moods of deep depression, which sometimes lasted for two or three days. As Bohr* put it, when speaking after a public dinner at which Rutherford was the guest of honour: "To the pupils in his laboratory it is often as if the sun suddenly began to shine when he arrives in the morning; but sometimes it is as if the sky was darkened by a thundercloud". On bad days he was apt to be unreasonable, and one avoided contact with him as far as possible, but he generally cheered up by tea-time, or earlier if he could be made to laugh, even against himself. I once failed to notice that he was in a bad mood, and I unwisely asked him to authorize the payment of a few shillings for new spring balances for the sonometers in the elementary laboratory. So badly had the time been chosen that this precipitated a tempest, and "Why must you always have money? Why can't you learn to improvise? Why don't you hang beakers on the ends of the wires and load them by pouring in water?" I must have been in an unusually obstinate mood myself, for I ventured to ask him to calculate the size of a beaker large enough to hold 25 pounds of water, and to guess its probable cost and almost certain fate. This did the trick; in a few seconds he laughed, and soon afterwards he was carolling "Onward, Christian soldiers" as he went on his round. As a matter of fact, his rare bouts of ill-humour did practically nothing to dispel the cheerful atmosphere which his normal good fellowship created and maintained in the laboratory, and his grumblings were

* Eve, *Rutherford*, p. 363.

usually so utterly unreasonable that it was difficult for anyone to take them very seriously, or to give them any personal application. When they were not unreasonable, they were made directly to the man concerned, they were usually salutary, they were not unnccessarily repeated, and I do not think they were often resented for long. Rutherford was a fairly hard taskmaster, but he spared himself no more than he spared others, and his laboratory housed a very happy family, with remarkably few subterranean rumblings against its head.

The only persistent rumbling had its origin in causes common to most universities of that time, and not peculiar to Rutherford's department. Manchester University was a very good university (and still is; I used the past tense only because I was speaking of a period in the past), but it was expanding rapidly, and it had little money to spare for salaries. Junior staff salaries in science were commonly £125–£150, and the term *junior* in this connection was used very elastically. Some men who had tastes they could not gratify on less than £3 a week, or who wished to marry, undoubtedly found Rutherford's attitude unsympathetic, and his views on the advantages of plain living and high thinking somewhat exaggerated. It is probable that his attitude was largely defensive, and that he was, in fact, putting the best face he could on his own lack of power to move against the general policy of the University, but I think it is true that he did show some lack of imagination in this respect. It should be added that his own personal tastes were extraordinarily simple, and that throughout his life he was almost fantastically indifferent to motives of personal gain. Money meant little to him, and he was perhaps always surprised to find that other people had uses for it. His own instincts were generous and hospitable; he could be generous even with his time, which he valued more than money, and he was surprisingly tolerant of the well-intentioned cranks who pester eminent scientists and of foolish amateurs who wanted to borrow radium, of which they did not appreciate the value, to exhibit at popular lectures. "Seal up some emanation residues in a glass tube with some dirt and a little zinc sulphide, and send it to them", he would say. "It will look like a million pounds worth of radium, and nobody will know the difference."

I have already mentioned the excessive humility of Rutherford's estimate of himself at the time of his Montreal appointment. This was naturally soon modified by the brilliant success of his work, and there is an unmistakable and very human note of triumph in the Preface to the second edition of his *Radioactivity* (1905), where he "apologizes" for bringing out what was "almost a new work" of 580 pages barely fifteen months after the issue of a first edition in 399 pages; he refrained from pointing out how much of the new material was directly due to his own activity. Before he left Montreal, he was indisputably supreme in his own subject; no imaginable share of humility could have prevented him from realizing this, and only an entirely misplaced modesty could have led him to deny it. He took the eminently sensible course of thoroughly and openly enjoying the fame he had earned and the occasions of its public recognition, and he liked to see his friends sharing his enjoyment. He was, however, innately and fundamentally much more modest about his achievements than was always apparent to the casual observer, and he made no pretence of special knowledge outside his own subject. I have a pleasant recollection of him,

seated in a class that contained many of his own students, industriously taking notes of a course of lectures on the theory of probability given by Professor Horace Lamb, and if Lamb had given us exercises to work out, I should not have been surprised to see Rutherford handing in his exercises with the rest. He had these other signs of essential modesty: he would not only defer to the opinions of authorities in other subjects, but he would also listen with every appearance of respectful attention to quite junior workers in his own subject, and even to people who were merely plausible humbugs; it is not suggested that he always allowed such people to influence his judgement.

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I fully realize that the account I have given may be indicted on the ground that it has dealt mainly with the more superficial aspects of Rutherford's work and personality. This choice of treatment has been made quite deliberately. There were, of course, depths and subtleties in Rutherford's mind that are far from apparent in the simplicity and directness of his final approach to and attack upon a physical problem. It is more true of Rutherford than of most men that we cannot

> "parcel out
> His intellect by geometric rules,
> Split like a province into round and square",

and I know that not only more, but vastly more, subtlety than I can command would still be inadequate for even a rough analysis of his genius.

The simplicity of his experimental methods was easy enough to observe. It was neatly summed up by Moseley in a few half-serious words at the end of a discussion on methods he and I were going to use for measuring ionization currents. We decided to use Townsend's induction balance, with which the electrometer deflection is nullified by regulation of a potentiometer circuit attached to a condenser. "But", said Moseley, "Rutherford will think it very effeminate of us to use a null method when we might measure the deflection instead". Similarly in his appearance and his life outside his work there were many things to suggest the same kind of deceptive simplicity—such as his large build, suggestive rather of latent than active power, his faintly bucolic appearance, his almost schoolboyish sense of fun, and even, in those days, his habitual pipe-smoking. He was a heavy smoker, and it was noticeable that, like many men who work constantly at high pressure, he burnt a great deal more tobacco than he smoked. He was also an inveterate borrower of matches (a venial sin in days when safeties could be purchased almost anywhere at a penny a dozen boxes), and his habit of pocketing the borrowed box was, so far as I can remember, his sole concession to the popular legend of professorial absent-mindedness.

He had read widely and well; just how widely was difficult to know, for he never had the habit of dragging in literary allusions by the coat-tails—they emerged only by chance, and at rare intervals. It was, however, a safe guess that when he turned to fiction for relaxation it would most probably, though not certainly, be to a simple narrative and not to an elaborately psychological

novel, and that he would revel in a book like *David Harum*. That he had a subtle and penetrating mind, working at times by ways that were quite inscrutable, was obvious to all who worked with him; its quality should be obvious to all who read his work. That he had a simpler side, which he liked to keep in the foreground, and which enabled him to enjoy simple things, was his and his friends' good fortune.

This lecture began with a quotation from Rutherford. It may fittingly end with a quotation from another great physicist, von Helmholtz, taken from an address * given almost exactly sixty years before the date of Rutherford's death. I am sure it will strike the same chord in any of Rutherford's men that it struck in me when I first read it over thirty years ago. Von Helmholtz was speaking of his own great teacher, Johannes Müller, and after stressing the importance to the student of "daily mental intercourse with teachers from whom he learns something of the workings of the thoughts of independent minds", he went on to say: "Anyone who has once come in contact with one or more men of the first rank must have had his whole mental measuring-rod altered for the rest of his life."

ACKNOWLEDGEMENTS

I have pleasure in thanking Dr. Norman Smith, Registrar of the Victoria University of Manchester, for compiling and sending to me lists of the workers in the Manchester laboratories, with the relevant dates, and the details of the grants made to the Physics Department for apparatus during Rutherford's tenure of the Langworthy Professorship. Also Dr. Norman Feather, for suggesting a slide of a portion of Rutherford's rough preliminary draft of the theory of the nuclear atom, for the loan of a suitable page of the manuscript and for authorizing its reproduction here. References to Eve's *Rutherford* and Feather's *Lord Rutherford* have, wherever possible, been indicated in the text.

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# A MULTI-PURPOSE COLLIMATOR

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## §1. INTRODUCTION

The object of this paper is to describe a collimator which has been found to be very useful for a number of purposes (when incorporated as one of the parts of an optical bench). With such a collimator the following types of measurement can be made:—

(1) radii of curvature of lens surfaces,
(2) equivalent focal length of a positive lens,

* Von Helmholtz, "On Academic Freedom in German Universities", October, 1877.

(3) back focal length of positive or negative lenses,
(4) measurement of certain prism angles ($\pi/4$ and $\pi/6$),
(5) interferometric tests of plates for parallelism.

The design of the collimator was made with a view to the inclusion in one single instrument of a number of well-known methods of measurement.

## §2. DESCRIPTION OF COLLIMATOR

The collimator, as far as its normal features are concerned, consists of an achromatic object-glass with a graticule placed in its focal plane, the graticule having divisions of 1/10th part and 1/100th part of the equivalent focal length of the object-glass, enabling measurements of the equivalent focal length of lenses to be made in the ordinary manner.

Additional features which are novel are as follows:—

About three-quarters of the way along the tube an additional eyepiece, carrying a half-silvered cube, is fitted as shown in figure 1.

The introduction of the half-silvered prism causes the focal plane of the collimator object-glass to be displaced further away from the object-glass,

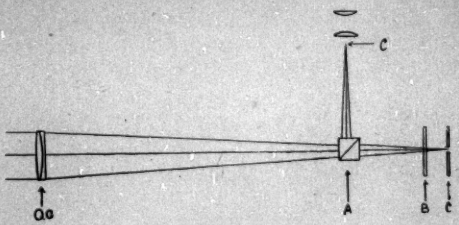


Figure 1.

A = Half-silvered prism.
OG = Object-glass.
B = Divided scale plate.
C = Pin-hole diaphragm.

and in this new focal plane is placed the second additional feature, which consists of a removable cell containing a stainless-steel mirror with a circular pin-hole in its centre. This provides at once an artificial star if the pin-hole is small and the corrections of the collimator object-glass are sufficiently good, but the main function of this feature is to turn the collimator into a spherometer, and for this purpose additional equipment is required in the form of supplementary lenses. These supplementary lenses are mounted in front of the collimator object-glass, thereby bringing the parallel beam to a focus. When the eye-piece and prism tube are inserted, and the additional lens fitted to the collimator, an image of any object which is placed at the focused image of the pinhole is seen in the eyepiece; for example, an image will be seen of the polished surface of a lens placed at this position, in so far as it is illuminated by the pin-hole.

## §3. FOCUSING THE COLLIMATOR

The focusing of the collimator may be accurately performed when the pin-hole mirror is in use, with the aid of a plane mirror placed nearly perpendicularly to the axis of the collimator object-glass. If the pin-hole is illuminated and the mirror is not quite perpendicular to the axis, the light after reflection is brought to a focus on the pin-hole mirror and reflected back again to, and through, the object-glass, to be again reflected and brought to a focus on the original pin-hole aperture. In the observing eyepiece there will then be seen two images of the pin-hole, one due to a double transmission through the object-glass, and one due to a quadruple transmission through the object-glass; and if the pin-hole mirror occupies the true focal plane of the collimator object-glass, these two images will occupy the same focal plane in the eyepiece.

The procedure then for focusing the collimator and adjusting the eyepiece so that it is conjugate to the pin-hole mirror is quite simple. The collimator object-glass is adjusted so that both images found in the observing eyepiece lie in the same plane, and the eyepiece is focused until both images are sharp. The plane mirror may now be adjusted so that both pin-hole images in the eyepiece coincide, when it will be set perpendicularly to the axis of the collimator.

## §4. MEASUREMENT OF RADIUS OF CURVATURE

The radius of curvature of a lens surface can be measured by means of the preceding set-up. The lens is mounted and the position noted at which the surface image is in focus. The lens is next moved until the focused image of the pin-hole coincides with the centre of curvature of the surface which is being measured; the difference of the two positions is then the desired radius of curvature, as indicated in figure 2.

If the surface of which the radius of curvature is to be determined is not truly spherical, then its departure from sphericity, if greater than one-quarter

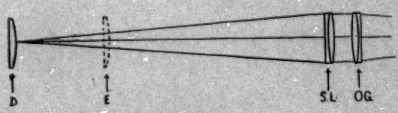


Figure 2.

OG = Object-glass.
SL = Supplementary lens.
D = First position of lens.
E = Second position of lens.

of a wave-length over the area which is illuminated by the beam from the collimator, will show as a marked deterioration in the image of the pin-hole as seen in the eyepiece, and this is a considerable advantage as *the surface is automatically tested for sphericity at the same time as its radius of curvature is measured.*

It is obviously desirable from all points of view that the numerical aperture of the supplementary lens used with the collimator should be of the same order of magnitude as the numerical aperture of the surface with respect to its centre.

## § 5. MEASUREMENT OF BACK FOCAL LENGTHS OF POSITIVE OR NEGATIVE LENSES

The set-up as used for spherometry is the most accurate for this measurement, but an optically flat mirror mounted perpendicular to the axis of the collimator is also required. The lens is mounted and the position noted at which the image of the pin-hole on the back surface is in focus. The lens is then moved until the focused image of the pin-hole coincides with the back

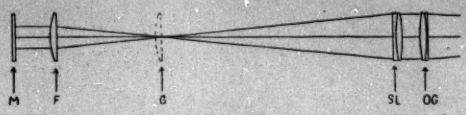


Figure 3.

OG = Object-glass.
SL = Supplementary lens.
G = First position of lens.
F = Second position of lens.
M = Mirror.

focal point of the lens. When this position is reached, the light emerges from the lens as a parallel beam, as is shown in figure 3, and the difference in positions gives the back focal length of the lens.

It is easily seen that both positive and negative lenses are dealt with with equal facility by this method.

## § 6. MEASUREMENT OF CERTAIN PRISM ANGLES

The deviation of the right angles from 90° in a nominal right-angled isosceles prism can be easily detected and measured. For this purpose the artificial star is used without the supplementary lenses, and the prism is placed on the lens-mount with the hypotenusal surface facing and perpendicular to the parallel beam of light, as shown in figure 4. Inspection of the image in the eyepiece

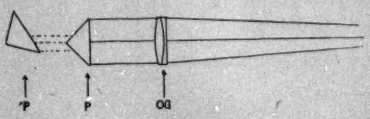


Figure 4.

OG = Object-glass.
P = Prism.
P' = Tilted prism.

will show three pin-holes for a prism which has a perceptible error, one of these images being a faint image due to the hypotenusal surface, while the other two, which are derived by internal reflections at both cathetal faces, will be bright. Rotating the prism will cause the faint image to traverse the field of view, while the two bright images will be stationary; if their separation is measured by means of an eyepiece graticule, and the refractive index of the glass is known,

the error can be calculated. At the same time the prism may be checked for pyramidal error; if this is present, and the prism is rotated, the faint image will traverse the field of view on a line parallel to the line joining the two bright images, and the separation between these two lines is a measure of the pyramidal error present.

The deviation of any or all of the angles of a nominally equilateral prism from 60° may be measured in a similar fashion, the images in this case all being faint images, but the general procedure being the same.

The departures from the nominal angles are $\delta/4\mu$ for a right-angled prism and $\delta/6\mu$ for a 60° prism, if $\mu$ be the refractive index of the prism and $\delta$ the separation of stationary images when the prism is rotated.

## §7. SETTING UP AND CENTRING A LENS

It is of interest to note that by means of this instrument a lens may be set up very accurately on an optical bench provided with a turntable and levelling table. The lens surfaces are brought in turn into the field of the eyepiece by traversing the lens along the optical bench, and adjusting the lens transversely and vertically until the images from each surface are equally bright. The lens is then traversed longitudinally until its centres of curvature come into the field of view and then, by rotating the lens table and using the levelling table, the lens may be accurately centred; further adjustments to the height and transverse position will usually be required. However, the lens can be set eventually so that the surface images and centre images all lie on one and the same straight line and come successively into view with a longitudinal movement.

## §8. CHARACTERISTICS OF THE VARIOUS PIN-HOLE IMAGES

When the plane reflecting mirror is used for measuring the constants of a lens an image will be formed in the eyepiece when the pin-hole image coincides with the focal point of the lens, and a further image will be seen in the eyepiece when the lens forms an image of the pin-hole image on the surface of the flat mirror. These images are additional to those seen by reflection at the centres of curvature and at the surfaces of the lens. The total number of images to be found in a normal set-up is therefore six, and they have the following characteristics:—

| | |
|---|---|
| 2 images by reflection at the surfaces ... ... | Colourless and erect. |
| 1 image by reflection at the centre of the surface nearest the collimator ... ... | Colourless and inverted. |
| 1 image by reflection at the centre of the surface furthest from the collimator and refraction throughout the near surface . | Coloured and inverted. |
| 1 image by reflection with parallel light from the end mirror... ... ... ... .. ... | Coloured and inverted. |
| 1 image by reflection when light is focused on end mirror ... ... ... ... ... ... | Coloured and erect. |

For a set of observations only three of these images will usually be required, giving radius of curvature of one surface and back focal length from one surface, the lens then being reversed to obtain similar data from the second surface. When the above details of the images are borne in mind, there is usually no difficulty in determining which three images are required out of the available six, although in this connection an unsymmetrical pin-hole might be an aid.

## § 9. INTERFEROMETRIC TESTS OF PLATES FOR PARALLELISM

With the aid of a light source having a bright line spectrum, such as a mercury-vapour lamp, plates may be tested for parallelism, the pin-hole being illuminated by the line source, and the plate being set perpendicularly to the optical axis of the collimator. If the eyepiece is removed and inspection made through the eyepiece tube, interference fringes, due to the combined reflections from front and back surfaces, may be seen and can be used to measure the lack of parallelism of a plate.

## § 10. USE OF A DIRECT-VISION PRISM IN COMBINATION WITH THE EYEPIECE

If a direct-vision prism is mounted between the half-silvered prism and the eyepiece, and a mercury-vapour lamp is used as a light source, a very small dispersion will suffice to separate the various coloured images owing to the small angle subtended by the pin-hole; in consequence, the chromatic aberrations of a lens may be measured accurately and rapidly without the loss of light and lack of precision which accompanies the more ordinary methods of measuring with colour filters.

## § 11. ACCURACY OF SETTING

This account of the procedure may be amplified a little at this point by some notes on further details of adjustments which may be made. In the determination of the back focal point, for example, the light will normally traverse the lens twice, so that this setting may be made with double the accuracy of an arrangement in which the light only traverses the lens once; but by a slight tilt of the end mirror, so that the reflecting pin-hole mirror comes into action, the light may be made to traverse the lens under test four times, two images being found in the observing eyepiece and the fainter one being sharply focused.

Similar precision may be obtained in the location of the centre of curvature of a surface provided the surface is silvered; otherwise there is insufficient light for visibility, but the lens must now be slightly rotated to provide two images in the eyepiece.

The location of the surface itself can only be accomplished with half the accuracy of the two immediately preceding cases, since the image in the eyepiece cannot be split into two by tilting the lens.

If the depth of focus of a lens system is determined from the Rayleigh limit, a figure or figures can be found giving the accuracy with which these settings may be made, and one has $d = \lambda/u^2$ if $\lambda$ = wave-length of light used, $u$ = semi-angle of collimator with supplementary lenses and $d$ = depth of focus.

Consequently for the optimum case

Accuracy in the location of a surface $= \lambda/2u^2$.
Accuracy in the location of a focal point $= \lambda/4u^2$.
Accuracy in the location of a centre of curvature $= \lambda/4u^2$.

These results are based on the assumption that the lens or surfaces have sufficient angular apertures to take in the whole of the collimator beam; they have been found in practice to agree with the accuracy obtainable with this instrument.

If the collimator is accurately focused for light of one colour, the chromatic aberration (i.e. secondary spectrum) of the object-glass will usually be apparent, but it can be shown that if one requires, for example, to make measurements of focal length for a range of colours it is unnecessary with this instrument to refocus the collimator for each colour provided the complete set of measurements (i.e. distance of focal point to surface) is made with light of one and the same colour for each colour. This is owing to the fact that the second power of the error of focus of the collimator is the lowest to be effective in the combined errors of surface location and focal-point location.

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# THE ADIABATIC TEMPERATURE CHANGES ACCOMPANYING THE MAGNETIZATION OF IRON IN LOW AND MODERATE FIELDS

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*ABSTRACT.* The new method devised for the measurement of the small heat changes which accompany step-by-step changes in the magnetization of ferromagnetic materials in fields of the order of a few hundred oersteds has been applied to the study of Armco iron in the annealed state and in the cold-drawn temper. Exceptional difficulties were encountered in the experiments, but the measurements appeared sufficiently reliable to permit a detailed analysis to be made of the energy changes in the several stages of the hysteresis cycle. Evidence was found for the existence of a kind of discontinuity in the magnetization of iron in fields of 350 to 400 oersteds predicted by Akulov and hitherto not observed experimentally.

## §1. INTRODUCTION

An extensive investigation of the temperature changes which occur when a ferromagnetic substance is taken through an ordinary or so-called "technical" hysteresis cycle was made by Bates and Weston (1941), and reported in their paper, hereafter referred to as Paper I, in which references to earlier work by other investigators are given. They confined their attention to work on nickel and certain nickel-iron alloys of particular interest to them, and iron was not studied in detail because the preliminary experiments which

they made upon it showed that the experimental difficulties were very much greater than those found with nickel. Indeed, it was then considered likely that accurate results for iron would never be obtained by the special method used by them. While it is still not possible to give an entirely satisfactory explanation here for this remarkable difference in the behaviours of iron and nickel, it can at any rate be said that the method is applicable to iron.

The Armco iron used in the work described here was kindly supplied by Professor F. C. Thompson in the form of rods approximately 0·36 cm. in diameter. The material was in the cold-drawn temper and it was investigated both in this state and in the annealed condition, the latter being obtained by maintaining rods *in vacuo* at a temperature of 950° c. for half an hour and then allowing them to cool slowly, a treatment recommended by Dr. Pfeil. The magnetic and thermal measurements were made by the procedure described fully in Paper I, so that it is necessary to give here only a bare outline of the method of measuring the small heat changes.

A 40-cm. rod was mounted vertically along the axis of a water-cooled solenoid which provided a field of 121·7 oersteds per ampere when supplied by a steady current which could be varied in discrete steps. Temperature changes of the rod were measured by means of twenty copper-constantan thermocouples. The "hot" junction of each couple was kept in loose contact with the rod, while the "cold" junction was very close to, yet thermally insulated from, the rod except for conduction along the material of the couple. Each couple was connected in series with its own primary winding of low-resistance copper wire wound upon a mu-metal spiral core or ring. A low-resistance secondary coil of many turns was wound upon this core and joined in series with a specially designed fluxmeter of high sensitivity. Twenty identical thermocouples were used, and as each one was in series with its own primary coil on the spiral core, electrical insulation between the "hot" junctions and the rod specimen was not necessary.

When the temperature of the rod was suddenly changed by a small increment $\Delta T$, a current strictly proportional to $\Delta T$ flowed in each thermocouple circuit and remained constant for a short interval of time. Each primary coil therefore contributed to a change of flux in the mu-metal core, and this change persisted as long as the thermocouple currents remained constant. Consequently, each change in temperature $\Delta T$ produced a corresponding change in flux and a corresponding ballistic deflection of the fluxmeter proportional to $\Delta T$. The arrangement was easily calibrated by producing standard adiabatic changes in temperature of the order of 0·001° c. by suddenly applying a longitudinal stress of $F$ dynes to the rod. Joule showed that the adiabatic fall of temperature $\Delta T_1$ was given by

$$\Delta T_1 = \frac{-\alpha T F}{J \rho s A} = \frac{-\alpha T F}{Q A},$$

where $\alpha$ is the coefficient of linear expansion of the rod, $T$ its absolute temperature, $J$ the mechanical equivalent of heat and $\rho$, $s$ and $A$ are respectively the density, specific heat and area of cross-section of the rod. Since the energy in ergs required to raise the temperature of 1 c.c. of the rod by $\Delta T$° c. is $Q \cdot \Delta T = \frac{-\alpha T F}{A}$,

the density and specific heat of the rod are not required in order to express experimental results in the most convenient form for hysteresis studies.

The magnetic hysteresis cycle was found by the usual ballistic method, using a search-coil wound on the middle portion of the rod and connected to a ballistic galvanometer. All measurements were standardized with a calibrated Grassot fluxmeter and a standard mutual inductance. The solenoid was mounted vertically and the vertical component of the earth's magnetic field was neutralized in the usual way.

## §2. EXPERIMENTAL DETAILS

The several sources of error peculiar to the method have been discussed fully in Paper I, and we took the same steps to avoid errors due to zero drift of the fluxmeter, loss of heat, eddy currents flowing in the specimen and the effects of stray fields from the specimen and solenoid upon the mu-metal core and upon the thermocouple leads. We need, therefore, mention here only special points of interest which emerged in the course of the present work.

The success of the method in the case of iron depended almost entirely upon the behaviour of the fluxmeter system. The same instrument was used as in Paper I, but owing to circumstances beyond our control the suspensions had to be renewed. During this operation opportunity was taken to align the moving-coil system more exactly. This was an unfortunate move, for it was found later that an asymmetric mounting of the coil with respect to the vertical sides of the pole pieces, almost as if one wished to provide a gravitational torque of the "self-closing gate" type, was essential to very high sensitivity, and it was extremely difficult to decide upon the precise degree of asymmetry required. It was also found that the position of the iron cores within the moving coil with respect to the vertical line had very great influence on the sensitivity and stability of the instrument. By rotating the cores slightly from the vertical, the controlling field was distorted so that the moving coil took up an equilibrium position resulting in a motion in a desired direction when the electromagnet coils were energized. It was suspected that the properties of the ferromagnetic control strips had deteriorated, but under existing conditions it was not possible to face the lengthy task of making and testing a new moving-coil system. The adjustment of the fluxmeter for maximum sensitivity and stability was always very critical.

Induced currents were bound to be set up in the several thermocouple circuits, but their magnitude remained somewhat a mystery. Their effects on the fluxmeter were eliminated by using a compensating coil, joined in series with a spare primary coil on the mu-metal core, and placed near the solenoid, as described in Paper I. The resistance of the complete compensating coil circuit was equal to that of a thermocouple circuit, so that the time factors were equal. Great care was given to the symmetrical adjustment of the thermocouple leads, so that induction effects could be self-annulled, and in this way the effects were certainly much reduced; but, even allowing for the high sensitivity of the fluxmeter, it was never fully understood how effects so suggestive of marked magnetic asymmetry could be set up. We are inclined to the view

that they were due to large variations in the magnetization and in the electrical conductivity of the surface from place to place.

Unfortunately, their magnitude varied from point to point on the hysteresis cycle, so that the position of the compensating coil had to be determined anew for each projected change in the magnetic state of the specimen. Although the necessary solenoid changes were chosen to give magnetic induction changes in the iron specimen of approximately the same magnitude as those used by Bates and Weston in the case of nickel, the disturbing effects were many times as large, and could only be neutralized by using a much bigger compensating coil, often very close to the solenoid, and some of its settings were extremely critical, especially when the total induction of the specimen was large.

Since the saturation magnetization of iron is much greater than that of nickel, the effects of eddy-current heating would be considerably greater unless the diameters of the iron specimens in these experiments were less than those of the nickel specimens. Our iron rods were approximately 0·36 cm. in diameter, while the nickel specimens of Paper I were between 0·45 and 0·50 cm. However, this adjustment of diameter did not cause eddy-current heating to be negligible, and a method of correcting for its effects is described below. Unfortunately, the lesser diameter of the iron meant that the area of contact between a thermo-junction and the rod was reduced and sensitivity thereby decreased.

Moreover, it has long been known that the magnetic induction of pure iron exhibits time-lag effects; hence it was necessary to ensure that each ballistic deflection of the fluxmeter truly represented the whole of the thermal change accompanying a change in magnetization. Accordingly, in many instances sensitivity was deliberately sacrificed to some extent in order to take advantage of enhanced stability of zero. The nett result was that whereas Bates and Weston normally worked with a sensitivity of $8 \times 10^{-6}$ °C. per mm. scale deflection, we had to be content with $18 \times 10^{-6}$ °C. per mm. on the average; in both cases it was possible to read to 0·1 mm.

Owing to the smallness of many of the fluxmeter deflections and the critical setting of the compensating coil, it was not expedient to make preliminary tests of the positions of the latter for a complete series of solenoid current changes, which were then carried out in a continuous manner according to the plan of Paper I. Instead, it was necessary to take many readings of the temperature change for one and the same step in magnetization, first finding the correct position of the compensating coil and then going quickly through the remainder of the complete cycle back to the start of the step ready for another determination, without attempting to determine the temperature changes for any other portions of the cycle. This had naturally the advantage that the sensitivity of the arrangement could be closely watched, and enabled a reliable average value of the temperature change for a given step to be found. On the other hand, it made the work much more tedious; and the testing for the correct position of the compensating coil until the initial jerky induction deflection disappeared with a given step was often lengthy enough.

It was pointed out in Paper I that, provided that the effects of induction on the thermocouple leads could be sufficiently reduced, they could be completely eliminated by taking half the sum of the fluxmeter deflections obtained

when a chosen step was traversed in the two possible directions. For example, if one described a hysteresis cycle with a maximum range of field from +200 to −200 oersteds, one would expect to obtain equal fluxmeter deflections on changing the field from +200 to +160 and from −200 to −160 oersteds under ideal conditions, with no induced current effects present. In general these effects were present, and the mean deflection was taken to represent the true thermal change, on the assumption that if induction effects were in the same sense as the thermal effects in the +200 to +160 change they would be in the opposite sense in the −200 to −160 change. It was rarely possible to wipe out induction effects entirely, and in one or two cases we obtained a marked asymmetry in the readings, to which reference is made below, when as far as could be seen no trace of the jerky fluxmeter deflection normally associated with uncompensated induction effects was present when the step was made in either sense.

In Paper I was given a very convincing proof of Warburg's law, which states that the energy dissipated in one complete hysteresis cycle is equal to $\oint_{+H}^{+H} HdI$. This could not be provided from our experiments on iron for several reasons. Firstly, while eddy currents were expected to produce effects of the same order of magnitude in both the iron and the nickel experiments, because of the smaller diameter of the iron specimens, they did in fact appear to play a much greater part in the case of iron, particularly in the annealed state. In the nickel experiments the hysteresis heat per cycle calculated from the area of the hysteresis cycle agreed with that measured directly in the thermal experiments within the limits of experimental error. In the case of iron the thermal measurements always gave values larger than the calculated ones, and a correction for this was applied to the thermal measurements as described below. Secondly, we could not be satisfied that all time-lag effects were overcome or that all induction effects were completely eliminated.

As the $I, H$ curve was found from measurements on a rod specimen, correction had to be made for the demagnetization effects of the free poles at the ends of the rod. The method of finding the demagnetization factor by the use of a coil wound on a horseshoe former, as described in Paper I, was employed, but it led to a value 0·000267, which appeared to us to be very low. Now an accurate knowledge of the demagnetization factor was not important in the nickel experiments, but in the case of iron the value of the remanent magnetization calculated from the experimental observations depended very markedly upon the demagnetization factor used for their correction. For example, in the cycle with a maximum field of 400 oersteds the annealed iron gave $I_r = 200$ and 1200 gauss respectively, using the lower and the higher demagnetization factor given below. We therefore made additional ballistic measurements on a rod bent to form an anchor ring, the ends of the rod being soldered in contact to avoid free poles. The two sets of measurements were in highly satisfactory agreement, but they gave values of the remanent magnetization which were far smaller than those recorded by other workers for Armco iron. While this fact does not necessarily mean that our values are incorrect, it did seem desirable, in order to compare our findings with those of others, to make additional calculations based on a demagnetization factor more nearly equal to that for

an ellipsoid of revolution of approximately the same dimensions as the specimen rod.

We therefore calculated this demagnetization factor by assuming that a properly corrected $I$, $H$ curve for well annealed iron would cut the $H$ axis at $H = -H_c$ at right angles. In other words, the tangent of the angle which the uncorrected $I$, $H$ curve made with the vertical at $H = -H_c$ was the required demagnetization factor. The value so found, 0·00458, was probably a little too high, but it was in very good agreement with the value for an elongated ellipsoid. A further justification in favour of the higher value is to be seen in figure 4, where the $\int_{-100}^{H} HdI$ curves calculated using both the demagnetization factors are drawn. In spite of the difference in the order of magnitude of the two demagnetization factors, it was practically immaterial which one was used in calculations for the unannealed iron specimen. Here, however, the remanent intensity of magnetization was between 730 and 745 gauss for maximum fields of 100 to 400 oersteds, but in the case of annealed iron specimens we found $I_r$ to be 1240, 1200 and 1190 gauss respectively for maximum fields of 100, 200 and 400 oersteds, whereas one would have expected the values of $I_r$ to be in the reverse order for the stated maximum fields.

## § 3. EXPERIMENTAL RESULTS

The Armco iron, a basic open-hearth ingot iron, had the following composition:—Iron 99·89, carbon 0·03, silicon 0·01, manganese 0·03, sulphur 0·02, phosphorus 0·02 per cent.

The main experimental results are set forth in the accompanying graphs, where figures 1*a* and 1*b*, 2*a* and 2*b*, and 3*a* and 3*b* refer to the experiments on the unannealed state for cycles in which the maximum fields used were 400, 200 and 100 oersteds respectively. Following the procedure of Paper I, in each graph the heat changes $\Sigma dQ$ are denoted by $Q$; these were obtained as the effective solenoid field was changed from the stated maximum value $H_m$ to the same maximum in the opposite sense, and are plotted either as a function of the intensity of magnetization of the specimen or as a function of the effective field. In order to economize in graph space, the values are plotted for one half-cycle only. In general, the changes in the remaining half-cycle can readily be obtained by rotating the existing $Q$ curve about the axis of ordinates and displacing it vertically until its starting point coincides with the position originally occupied by its end point. In the same figures the value of $\int_{-H_m}^{H} HdI$, etc., are plotted against $I$, together with the corresponding curves of $\left(\int_{-H_m}^{H} HdI\right) - Q$. The demagnetization factor used in these graphs was the higher value 0·00458.

It was stated earlier that we were unable to confirm Warburg's law in this work, there being a discrepancy between the energy dissipated per cycle calculated from the thermal measurements calibrated by loading and that calculated from the area of the hysteresis curve. We assumed that the discrepancies were due to the effects of eddy currents, and eliminated them in the

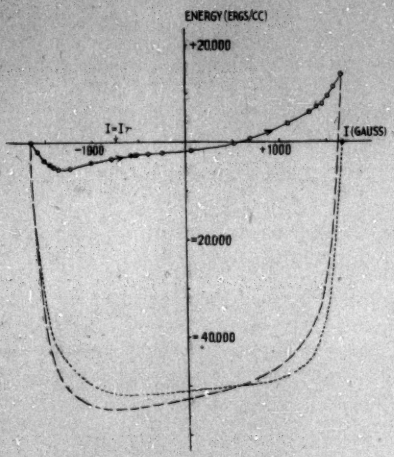


Figure 1 *a*. Unannealed iron.
Maximum field 400 oersteds.
—— $Q, I$; — — — $\int HdI, I$;
- - - $\int HdI - Q, I$.

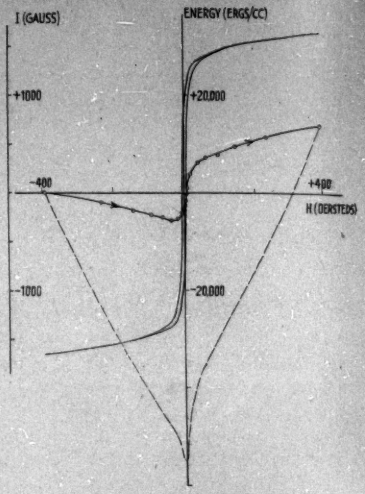


Figure 1 *b*. Unannealed iron.
Maximum field 400 oersteds.
Hysteresis cycle, $Q$, $H$ and $\int HdI$, $H$ curves.
—— $Q, H$; — — — $\int HdI, H$.

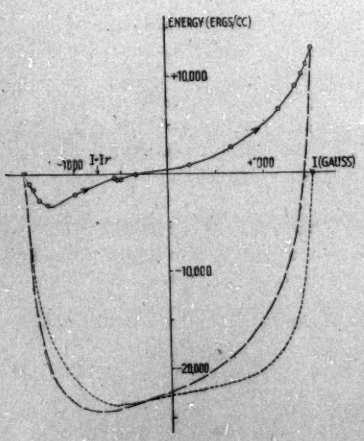


Figure 2 *a*. Unannealed iron.
Maximum field 200 oersteds.
—— $Q, I$; — — — $\int HdI, I$;
- - - $\int HdI - Q, I$.

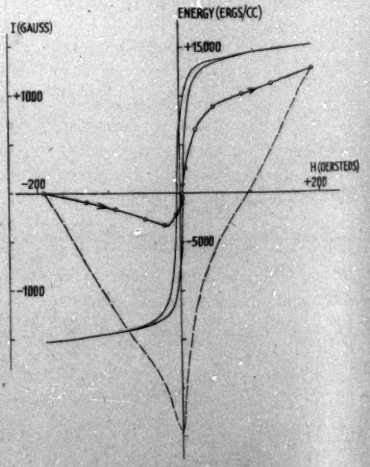


Figure 2 *b*. Unannealed iron.
Maximum field 200 oersteds.
Hysteresis, $Q$, $H$ and $\int HdI$, $H$ curves.
—— $Q, H$; - - - $\int HdI, H$.

following way. For each step in solenoid current the change in magnetic induction $\Delta B$ was measured, and it was assumed that the eddy-current energy liberated in the specimen with each step was proportional to $\Delta B^2$. The sum of the values $\Delta B^2$ for a complete cycle, i.e. $\Sigma \Delta B^2$, was therefore equivalent to the difference in the two energy determinations per cycle, and hence the energy corresponding to each of the quantities $\Delta B$ was found and the curves corrected accordingly. All the curves for $Q$ have been corrected in this way, so that the end points of the $Q$ and $\int HdI$ curves coincide, and in the case of annealed iron, where the correction is large, both corrected and uncorrected curves are shown.

The experimental results for annealed Armco iron are shown in figures 4, 5 *a* and 5 *b*, 6 and 7, where the differences in the energy scales of the several curves

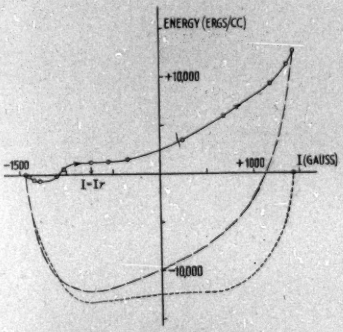


Figure 3 *a*. Unannealed iron.
Maximum field 100 oersteds.
—— $Q$, $I$; — — — $\int HdI$, $I$;
- - - $\int HdI - Q$, $I$.

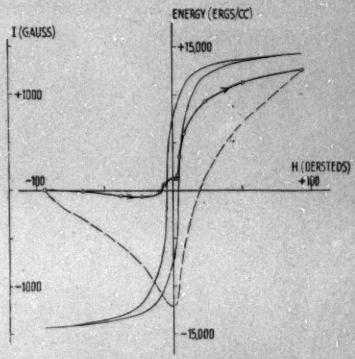


Figure 3 *b*. Unannealed iron.
Maximum field 100 oersteds.
Hysteresis, $Q$, $H$ and $\int HdI$, $H$ curves.
—— $Q$, $H$; — — — $\int HdI$, $H$.

should be noted. In figure 4, curve (1) shows the experimental values of $\Sigma dQ$ plotted as a function of $I$ without applying correction for eddy-current effects. Curves (3) and (4) show the values of $\int_{-100}^{H} HdI$ against $I$ obtained from the experimental $I$, $H$ curves corrected by using the larger and the smaller demagnetization factor respectively. The end points of these curves coincide because the area of a hysteresis cycle is not appreciably affected by the choice of demagnetization factor used in correcting the recorded values of the magnetizing fields. The difference between the end points of curves (1) and (3) on the right-hand side of figure 4 was attributed to the effects of eddy currents, and on correction for these effects, as described above, curve (2) was obtained.

The three points indicated by the filled circles on curves (1) and (2) were obtained by changing the solenoid current suddenly from $-0{\cdot}0150$ to $-0{\cdot}0045$ amp., from $-0{\cdot}0045$ to 0 and from 0 to $+0{\cdot}0045$. The change from

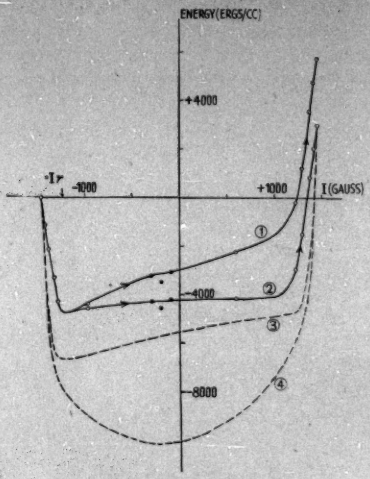


Figure 4. Annealed iron.
Maximum field 100 oersteds.

Curve (1) : Uncorrected $Q$, $I$ curve. Curve (2) : $Q$, $I$ corrected for eddy-current effects.

Curve (3) : $\int HdI$, $I$ curve, based on larger demagnetization factor.

Curve (4) : $\int HdI$, $I$ curve, based on smaller demagnetization factor.

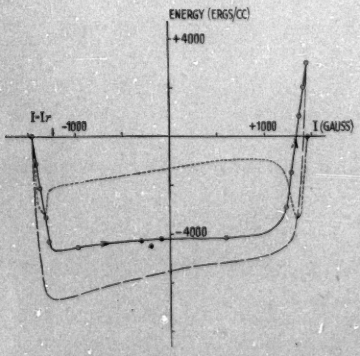


Figure 5 *a*. Annealed iron.
Maximum field 100 oersteds.

—— $Q$, $I$ corrected ; — — — $\int HdI$, $I$ ;

- - - $\int HdI - Q$, $I$.

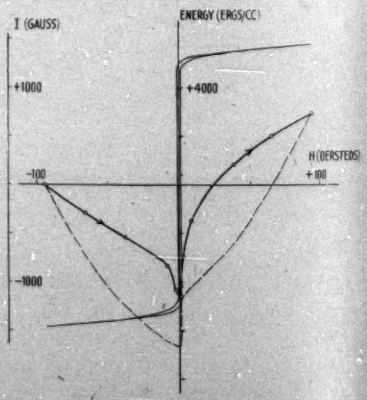


Figure 5 *b*. Annealed iron.
Maximum field 100 oersteds.

Hysteresis, $Q$, $H$ and $\int HdI$, $I$ curves.

—— $Q$, $H$ ; — — — $\int HdI$, $H$.

$-0{\cdot}0045$ amp. to 0 could be made in one of two ways: the solenoid circuit could be broken or the solenoid could be short-circuited by a mercury key of very low resistance. The second way was used in obtaining the readings plotted in figure 4, for it was found that if the solenoid circuit was broken, the change represented by the lowest filled circle was considerably more pronounced. In other words, the sudden removal of a small field by opening the solenoid circuit appeared to produce a pronounced cooling of the specimen. On remaking the solenoid circuit to give a small field of the same magnitude, but opposite sense, there was an equally pronounced heating. By short-circuiting the solenoid instead of breaking the circuit, the rate of change of the field was decreased and eddy-current effects made less intense, so that the cooling should have been more pronounced, but this was contrary to what was observed. Gerlach (1926) and Wolman (1928) state that the coercivity and remanence of single-crystal specimens depend greatly on the rapidity with which the magnetizing field is changed, the more rapid the change the less being the coercivity and remanence. Wolman points out that the latter phenomenon is also found with commercial specimens of iron, and he draws attention to the possibility of electrical surges and electrical and mechanical vibrations when the solenoid circuit is broken.

Now, a very sharp minimum is found in Ellwood's (1930) curves of $Q$ against $H$ for hard steel, and these curves have been criticized as likely to be more representative of magnetostriction than of thermal effects, because of the way in which Ellwood's composite specimen and thermocouple system was mounted. It is known that the transverse magnetostriction changes in iron taken through a hysteresis cycle are small at $H=0$ (Fricke, 1933), while longitudinal changes under similar conditions are pronounced (Bond, 1931). In fact, in the region of $H=0$ one would expect the specimen to contract in length at the maximum rate with respect to change in the applied field and to reach minimum length at $H=+H_c$, i.e. when the coercive field was applied. Because of demagnetization due to the free ends, the lowest filled circle is at approximately $H=+H_c$, although it was obtained by reducing the solenoid current to zero.

It was finally concluded that we were dealing with a spurious effect due to vibration, and we normally avoided it by proceeding from a small negative value of the solenoid current to a small positive value in a single operation, which meant that the specimen was prevented from remaining contracted for any appreciable interval of time. If the effect were entirely magnetostrictive in origin we would expect to observe a heating instead of a cooling on opening the solenoid circuit. It is much more likely that the sudden changes caused by breaking the circuit cause boundary displacements between the domains in the specimen which would not otherwise take place.

Other special features in the figures require comment. Curve (2) of figure 6 was obtained from curve (1) after correction for eddy-current effects. From $I=-1000$ to $I=+1000$ gauss it has no appreciable slope. This is in contrast to the results of Hardy and Quimby (1938) for annealed Armco iron with the same range of applied field. Their curve slopes markedly upwards to the right. The slope of curve (2) is, however, in good accord with the view that the large changes of $I$ over the flat portion are due to irreversible 180-degree boundary changes, in which little energy is dissipated (Bates, 1941).

A strange phenomenon was encountered in the case of the determinations plotted on the outermost sides of the $Q$ curves of figures 6 and 7. Normally, when the induction disturbances had been eliminated by means of the compensating coil, deflections of the fluxmeter for a given field change were in the same sense and equal to one another within the limits of experimental error for the two directions in which the particular half of the hysteresis cycle could be described, i.e. with the north-seeking or the south-seeking end of the magnetized specimen uppermost. Here, however, a marked asymmetry was found, one deflection being comparatively large and the other insignificant. For example, in one experiment we found that on reducing the solenoid current

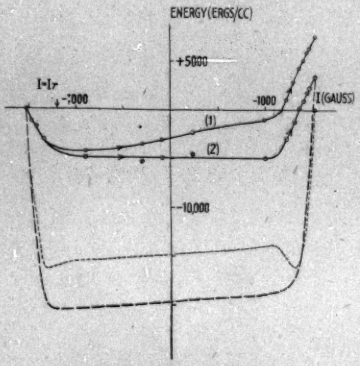


Figure 6. Annealed iron.
Maximum field 200 oersteds.
Curve (1), $Q$, $I$ uncorrected.
Curve (2), $Q$, $I$ corrected for eddy-current effects;
— — — $\int HdI, I$; - - - $\int HdI - Q, I$.

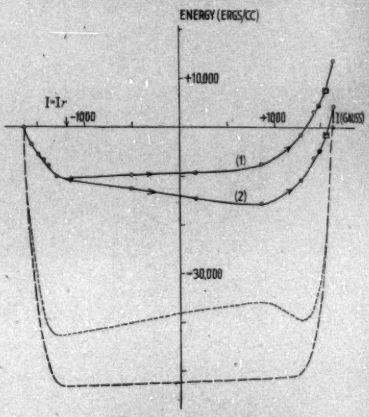


Figure 7. Annealed iron.
Maximum field 400 oersteds.
Curve (1), $Q$, $I$ uncorrected.
Curve (2), $Q$, $I$ corrected for eddy-current effects.
— — — $\int HdI, I$; - - - $\int HdI - Q, I$.

from 1·630 to 1·135 amp. the fluxmeter deflection was 0·27 cm. when the north seeking end of the specimen was uppermost and 0·00 cm. when the south seeking end was uppermost. This may be an example of the fact that annealing does not produce a random distribution of the crystal axes in the material, but merely shifts the preferred orientation, a fact discovered by Williams (1937) in experiments on the directional properties of unannealed and annealed discs of rolled nickel. Annealing does not usually produce magnetic isotropy, but changes the direction of easy magnetization. The present results would indicate that in our annealed specimen, rotation of the domain vectors took place more easily in one direction than in the opposite one, a state of affairs which it would be difficult to explain except on the basis of a mechanism like that suggested in the next paragraph.

Additional information is provided by the first and second points from the end on the right side of the $Q$ curve of figure 7, which are plotted as a square and a filled circle respectively. The fluxmeter deflections for the first point initially recorded, after the iron was thought to be in a cyclic state, were $+1{\cdot}71$ and $+1{\cdot}00$ cm. respectively for the two senses in which the hysteresis cycle was described; yet, after the cycle had been described many more times in recording other points on the $Q$ curve, repetition gave $+0{\cdot}37$ and $+0{\cdot}35$ cm. Again, in the case of the filled-in circle, the successive pairs of fluxmeter readings for $dQ$ were as under:—

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| (*a*) | $+0{\cdot}54$ | 0·51 | 0·42 | 0·29 | 0·33 | 0·28 | 0·25 | 0·21 | 0·21, |
| (*b*) | $+0{\cdot}17$ | 0·13 | 0·13 | 0·19 | 0·16 | 0·13 | 0·06 | 0·10 | 0·11, |

showing that the deflections, especially for one sense of change, fell considerably, although one would certainly have thought the iron to be in a stable cyclic state from the point of view of $I$, $H$ measurements. It was possible that persistent magnetic reversals had caused a hardening of the material, and we finally recorded $+0{\cdot}21$ and $+0{\cdot}11$, with a mean value of $+0{\cdot}16$, as the value of $dQ$ for this point. This unusual behaviour gave us much concern, for we were frequently in doubt as to the trustworthiness of individual readings, especially as the solenoid current was heavy and there was a tendency for the fluxmeter zero to wander, due to changes in the cooling water.

Now it is significant that in the case of pure carbonyl iron, magnetic time-lag phenomena are found to a marked extent only when, e.g., the material is first maintained in hydrogen at 1000° c. and then allowed to cool slowly (Richter, 1937–38), a treatment which is essentially annealing. Richter suggests that phenomena observed by him can be explained if we assume that magnetic time-lag is a manifestation of the effects of mechanical fatigue on magnetostriction phenomena. Every time the applied magnetic field is changed, magnetostrictive stresses are set up in the specimen and tend to oppose changes in magnetization. When these stresses gradually disappear, owing to plastic deformation of the strained regions, the magnetization changes accordingly. Richter produces evidence that the magnetic time-lag effects discussed by him are completely independent of ordinary hysteresis phenomena. The suggested mechanism would be of particular importance in those regions of the hysteresis cycle where stresses are most pronounced, namely, the regions in which rotations of domain vectors take place from parallel to the crystal axes to the direction of the applied field. Mechanical fatigue would account for the observed decrease in the fluxmeter deflections with increase in the number of times the cycle is described. Indeed, it may be that we ought to have traversed the cycle according to a regular time schedule. The great asymmetry of the deflections obtained when the cycle is described in opposite senses cannot be explained by mechanical fatigue alone. The directions of the eddy currents are opposite in the two cases, and it may be that a phenomenon analogous to rectification is present in the strained material.

Akulov (1931) was the first to draw attention to a remarkable discontinuity which ought theoretically to exist in the $I$, $H$ curve for a single crystal of iron when it is magnetized by a field parallel to a [111] direction. The predicted

discontinuity, a bending of the $I, H$ curve towards the $I$ axis, should occur with a field of the order of 350 to 400 oersteds, but it has never been observed experimentally, presumably because of the disturbing effects of internal strains in single crystals. However, the field range in which it ought to occur is precisely that in which we found unexpected phenomena, which we think could be explained on these lines.

## §4. DISCUSSION OF RESULTS

Many outstanding features of the results have already been discussed above, but a few points warrant further treatment. Measurements on annealed and unannealed Armco iron were made by Hardy and Quimby (1938) using material 99·43 % pure. Their $Q, I$ curves show marked similarities to the corresponding ones reproduced here for cycles with approximately the same maximum fields of about 200 oersteds. The main difference is found in the middle portions of the $Q, I$ curves for the unannealed material, as in Hardy and Quimby's curve the middle portion is much more nearly linear than was found here, and no mention is made of anything approaching the disturbance found at $I = I_r$ in our experiments.

One effect of annealing was to make the middle portion more linear, as is shown by figures 3*a* and 5*a*. Another effect was the decrease in the positive slope of the middle portion of the $Q, I$ curve—i.e. of the rate of change of $Q$ with $I$—as the maximum field of the cycle was raised. This decrease can be attributed to the overlapping of rotation processes and boundary extensions brought about by the use of higher fields.

A comparison of figure 1*a* with figure 7 of Paper I shows that the $Q$ curves of unannealed iron and unannealed nickel are surprisingly alike, especially when a high field is used in obtaining the iron hysteresis cycle; as the magnetostrictive properties of iron and nickel are very different, this would indicate that magnetostriction effects do not appreciably determine the course of the $Q$ curves. As we are dealing only with specimens which are not subjected to applied mechanical forces, we may write that the energy $HdI$ supplied to a ferromagnetic in any part of a hysteresis cycle is accompanied by an increase in internal energy $dE$ according to the equation

$$HdI = (\partial E/\partial I)_T dI + (\partial E/\partial T)_I dT.$$

It is the second term on the right-hand side of this equation which represents that part of the internal energy which manifests itself as an increase in temperature and which is denoted by $dQ$ in our measurements. Hence we may write $(\partial E/\partial I)_T dI = HdI - dQ$, independently of whether the change is thermodynamically reversible or not. The $\left(\int HdI - Q\right), I$ curves, or $(dE/dI)_T dI, I$ curves for hard-drawn iron and nickel are, however, very dissimilar, those for iron falling well below the $Q$ curves, while those for nickel cross the $Q$ curves. The importance of the difference curve is that its slope at any point gives the value of $(dE/dI)_T$ for the corresponding point on the $I, H$ curve. As we may write $E = E_c + E_F$, where $E_c$ is the crystal energy for unit volume and $E_F$ is energy per unit volume associated with strain of internal origin, we thus have information

of the way $(E_c+E_F)$ varies at any point on the $I$, $H$ curves independently of whether the variation is thermodynamically reversible or not.

All the difference curves for unannealed iron have the same general shape, and there is a minimum at $I=I_r$, an experimental result previously noted for hard-drawn nickel (Bates, 1941). As, however, $H=0$ at $I=I_r$, this really means that $[\partial(E_c+E_F)/\partial T]_I=0$ at $I=I_r$. The results with annealed iron do not seem to confirm this statement as accurately, but it appears to be satisfied within the limits of experimental error, for the value of $I_r$ is not known with certainty. In any case our work indicates that the magnetization processes in annealed iron are much less clearly defined than had previously been thought. Perhaps further elucidation would be obtained by repeating our measurements with the specimens under severe load, in spite of the additional factor thus introduced. Unfortunately, such studies have to be left for the return of more normal times

Perhaps the most important result of the present work is that it provides the first direct experimental proof of the existence in iron exposed to applied fields of 350 to 400 oersteds of a magnetic state wherein the energy relationships are by no means clearly defined (Bitter, 1937). Such a state was predicted on the basis of the theory of ferromagnetic anisotropy developed by Akulov and others, which has been discussed by Bates (1939), Bitter (1937), Stoner (1934) and Becker and Döring (1939). According to this theory we write

$$E_c=K_0+K_1(\alpha_1^2\alpha_2^2+\alpha_2^2\alpha_3^2+\alpha_3^2\alpha_1^2)+K_2(\alpha_1^2\alpha_2^2\alpha_3^2),$$

where $\alpha_1$, $\alpha_2$ and $\alpha_3$ are the direction cosines of the intensity of magnetization with respect to the axes of an iron single crystal and $K_0$, $K_1$ and $K_2$ are anisotropy constants. In the case of iron, $K_2$ cannot in general be neglected (Gans and Czerlinsky, 1932), but by neglecting the $K_2$ term and the effects of internal strains, a relation may be deduced between the intensity of magnetization, $I_{111}$, and the applied field, $H_{111}$, along the [111] direction, which shows that $dI_{111}/dH_{111}$ becomes infinitely great when $H_{111}$ is equal to about 350 oersteds. As Akulov pointed out, the magnetization processes in such a region cannot be reversible, and special hysteresis phenomena must therefore be expected in this region; our experiments provide ample confirmation of these views.

## § 5. ACKNOWLEDGEMENTS

Much of the apparatus used in this research was purchased from a grant made to L. F. B. by the Government Grant Committee of the Royal Society. Our thanks are also due to Professor F. C. Thompson for the gift of the Armco rods and to Dr. L. B. Pfeil for advice on annealing; D. R. H. desires to thank University College, Nottingham, for a Research Scholarship.

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# A PROBLEM IN TWO-DIMENSIONAL FLOW

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*MS. received* 30 *November* 1942

*ABSTRACT.* A formal solution is given for a two-dimensional flow problem noted by Macey (1942).

## § 1. INTRODUCTION

In a paper under the above title, Macey (1942) shows that the differential equation governing the movement of moisture in a porous material, when the flow is linear, is $\frac{\partial M}{\partial t} = \frac{\partial^2 M}{\partial x^2}$, and that when the flow is radial, the equation is $x\frac{\partial M}{\partial t} = \frac{\partial}{\partial x}\left(x\frac{\partial M}{\partial x}\right)$, where $M$ is the moisture content per unit volume at the quasi-time $t$ (a linear function of the time), and $x$ is a length co-ordinate.

In order to have the advantage of dimensional arguments, I prefer to alter the variables slightly and to write the equations as

$$\frac{\partial M}{\partial t} = \alpha \frac{\partial^2 M}{\partial x^2} \qquad \ldots\ldots(1)$$

and

$$\frac{\partial M}{\partial t} = \alpha\left[\frac{\partial^2 M}{\partial x^2} + \frac{1}{x}\frac{\partial M}{\partial x}\right], \qquad \ldots\ldots(2)$$

where $\alpha$ = flow per unit area/gradient of $M$.

Macey gives an explicit solution of (1) for the boundary conditions

$$\left\{\begin{array}{l} \partial M/\partial x = -A \text{ at } x = l \\ \partial M/\partial x = \ \ 0 \text{ at } x = 0 \end{array}\right\}$$

and the initial condition $M = M_0$ at $t = 0$, these conditions corresponding to the case of a bar initially with uniform moisture-content $M_0$, which loses moisture at a constant rate over the end $x = l$, and is protected from loss elsewhere.

He remarks that the corresponding solution for radial flow (in which

$x=0, x=l$ correspond to the axis and outer surface of a rod) is unknown, and he gives a solution of a second type. The purpose of the present note is to supply the solution of the form corresponding to the first type.

## § 2. SOLUTION

Since equation (2) is linear, the complete solution may be built up by the addition of particular solutions, of which $e^{-n^2\alpha t}J_0(nx)$ and $e^{-n^2\alpha t}K_0(nx)$ are known to be examples. In the present case, we must reject the Bessel function of the second kind, since it is infinite at $x=0$. The problem also differs from most diffusion problems, in that a term linear in the time is permissible. This is usually excluded by the requirement that the solution shall hold for all values of $t$, but since the extraction of moisture is in this case at a constant rate, the solution has only to hold until drying is complete at some point. To satisfy the differential equation, a term in $x^2$ must be added to balance that in $t$, so that the solution takes the form

$$M=C+Dt+Dx^2/4\alpha+\Sigma Be^{-n^2\alpha t}J_0(nx), \qquad \ldots\ldots(3)$$

with the initial and boundary conditions

$$\partial M/\partial x = \quad 0 \text{ at } x=0,$$
$$\partial M/\partial x = -A \text{ at } x=l,$$
$$M = M_0 \text{ at } t=0.$$

The boundary conditions are both satisfied if

$$J_1(nl)=0 \text{ and } D=-2A\alpha/l, \qquad \ldots\ldots(4)$$

whilst the initial condition requires

$$\Sigma B J_0(nx)=(M_0-C+Ax^2/2l). \qquad \ldots\ldots(5)$$

The constant $C$ is determined* by the requirement that the total loss of moisture in a time $t$ shall be equal to that flowing through the boundary in the same time, i.e.,

$$2\pi l\alpha\left(\frac{\partial M}{\partial x}\right)t=\pi l^2M_0-2\pi\int_0^l xM\,dx,$$

whence $C=M_0+Al/4$.

Multiplying (5) by $xJ_0(n'x)$ and integrating from $x=0$ to $l$, making use of the orthogonal property of the Bessel functions, we find

$$B=\frac{2A}{n^2lJ_0(nl)},$$

so that the final expression for the moisture content is

$$M=M_0-\frac{2A\alpha t}{l}-\frac{Ax^2}{2l}+\frac{Al}{4}+\frac{2A}{l}\Sigma\frac{e^{-n^2\alpha t}J_0(nx)}{n^2J_0(nl)}. \qquad \ldots\ldots(6)$$

This is the solution required.

## REFERENCE

MACEY, 1942. *Proc. Phys. Soc.* **54**, 128.

* I am indebted to Mr. Macey for pointing this out.

# THE MEASUREMENT OF DYNAMICAL MAGNITUDES

By NORMAN CAMPBELL, Sc.D., F.Inst.P.,
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*MS. received* 7 *December* 1942

*ABSTRACT.* A discussion is given of the question on what experiments the Newtonian theory of dynamics is most suitably based, and in particular whether quantity of matter, mass, and force can be measured independently of that theory.

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§1. These notes are inspired by a recent discussion of a paper by C. W. Hansel entitled "Fundamental laws and definitions in physics" (*Proc. Phys. Soc.* **54**, 509, 1942). The contributors to the discussion were mainly pedagogues, and the question that interested them was how physics should be taught to elementary students. I am not entitled to any opinion on that question. Nevertheless, and especially since the original paper had a wider title, it is permissible for me to point out that certain matters, apparently relevant to the discussion, were not fully considered, and that there is evidence that some of the contributors had definitely false ideas concerning them. On the other hand, in other matters I shall merely repeat what other contributors have already said.

Thus, students are apparently to be told (quite truly) that weighing (i.e. balancing in air) does not measure either mass or weight. But there seems to be no agreement on what they are to be told that it *does* measure. Indeed, I suspect that some contributors would tell them that weighing does not really measure anything at all, and would leave their students to be mystified when they discover that all the most precise measurements of the masses of terrestrial bodies actually depend upon weighing. Their policy is probably based on a sincere belief that no measurement can be truly *right* unless it measures a magnitude of ultimate theoretical significance. That doctrine is, of course, a fundamental part of Eddington's philosophy (dominating his paper just before the discussion, p. 491), but that is probably not why it is accepted. The reason is rather that it is not realized that every process of measurement contains its own test of rightness; that weighing is right because it satisfies the tests for "fundamental" or "independent" measurement, and leads to the result that two and two make four.* The rightness of weighing is independent of any

* Here, of course, I am referring to the description of measurement adopted in my *Physics: The Elements* (Camb. Univ. Press, 1920) and *Measurement and Calculation* (Longmans, 1928). Since these works appear to be generally incomprehensible, it may be well to point out that most of the description relevant to the present notes is contained in two recent short papers on "Dimensions and the facts of measurement" (*Phil. Mag.* **33**, 761, 1942) and "A demonstration of independent measurement (*Proc. Phys. Soc.* **55**, 98, 1943).

theory. Theory may tell us why (if weighing is not by substitution) the arms of the balance have to be equal; but the fact that they have to be equal and the decision whether they are or are not equal in any given balance can be and are best derived from suitable weighings.

§2. Apart then from all theory, weighing in air determines a definite magnitude; in order to avoid prejudice let us call it MW. The question then arises why MW is important in dynamics, if it is not any of the principal dynamical magnitudes. The proper answer today is: because, in conjunction with other magnitudes, it provides the most accurate means of measuring mass; but historically the significance of MW is that it was thought to be a measure of quantity of matter (QM).

The distinction between quality and quantity is perhaps based ultimately on such considerations as these. I have two blocks of gold, A and B, that share certain properties $X$ but are not identical. I find that I can carve out of B a block identical with A, leaving a residue C that still has the properties $X$. I then say that A and B both have the quality of gold, but that B contains more gold, a greater quantity of gold, than A. Quantity based on such considerations is quantity of a particular form of matter; quantity of matter, a more general conception, arises from mixing matter of different qualities. If I mix (e.g. by fusion) a block of silver and a block of gold, I get a block that differs from either of the original blocks in some (not all) of the ways in which B differs from A; there is in a sense *more* of the composite block than of either of the components. Thence, through several steps that need not be traced in detail, I am led to ask whether there is any property of all bodies that is "conserved" when they undergo certain changes, and in particular a change of "shape"—a term which will be used to include severance into and uniting of fragments. Can we find a property such that, if we measure it for a collection of any number of bodies, whether they are the same or different in quality, and measure it again for the whole of anything that results from changes of shape of those bodies, then the result of the second measurement will be the same as that of the first? If such a property can be found, it may be appropriately termed quantity of matter.

§3. This conception of QM is very ancient; Archimedes certainly possessed it when he tackled the problem of the crown. Unless he had possessed it, he could not have asked whether the goldsmith had stolen some of the gold given to him by the king; still less could he have asked whether the goldsmith had committed the more ingenious fraud of replacing some of the gold by an equivalent amount of silver. Moreover, he knew how to measure QM; the problem that he solved in his bath would not have arisen unless he had ascertained by experiment that the quantity of matter in the crown was the same as that in the king's gold; he would not have known that the more ingenious fraud, if any, had been committed. Though the story is not (I believe) explicit on this point, there is no doubt that he determined QM by weighing; he believed that QM was measured by MW.

Today we know that he was wrong. MW is not a true measure of QM, nor is (as he believed) volume, which in his bath he discovered how to measure

for irregularly shaped bodies. (We need not stop to inquire why the discovery of another way of measuring QM helped him to solve the problem.) And we know now that, when he cried "Eureka!", he had laid the foundation on which ultimately the discovery of the difference between QM and MW would be based. For his observation in the bath must have led to his work on hydrostatics, and it is from hydrostatics that we know that MW must depend on the density of the air in which the weighing is made, and cannot, therefore, measure QM, which is independent of that density.

Nevertheless, even if he had been able to foresee all the implications of his discovery, Archimedes would have been right to continue to regard MW as a measure of QM. For he did not recognize gases as matter; and, so long as he confined himself to solids and liquids, his balances were insufficiently sensitive to enable him to distinguish between MW and QM, that is to say, to show that MW is not always conserved in changes of "shape". Insensitiveness due to irregular errors is an integral part of measurement; and, so long as we are dealing with experimental facts only and are not concerned with theory, to say that measurement is affected by some systematic error that is completely masked by irregular error is to talk nonsense.

Nowadays our measurements are more precise; we can demonstrate experimentally that MW is not an accurate measure of QM, and that, in order to measure QM, we must apply a "correction" to MW, usually described as reducing the weighings to vacuum. But corrected MW (CMW) is still a measured quantity; the magnitudes entering into the correction are all measurable. The proper correction was doubtless discovered by the aid of theory, but it can be justified empirically; it could be shown by experiment that if, and only if, MW is corrected in a particular way, CMW is conserved. Further, we can now define much more precisely the circumstances in which CMW is conserved, and recognize that it is sometimes not conserved. There is, therefore, still a measurable property QM, defined by the proposition that it is conserved over a wide range of important changes. The transition from Archimedean to modern ideas was, of course, gradual and, as usual, illogical. But there does not seem ever to have been a time at which it would have been impossible to demonstrate the "conservation of matter", and, therefore, illegitimate to identify QM, the conserved property, with some experimentally measurable magnitude. For as soon as it was realized that MW and QM were different, CMW was available.

Before we pass on we must notice a feature of QM that is the cause of the complexity of its history. Usually as soon as we suspect the existence of a magnitude, and before we have actually measured it, we can guess the relation of equality appropriate to that magnitude; indeed, the relation of equality is inseparable from the very meaning of the magnitude. If there is a corresponding process of addition, so that the magnitude is measurable independently, it may appear at the same time, or may need to be sought for. But while the meaning of QM, namely conservation in changes of "shape", indicates at once the process of addition, it gives no guide to the relation of equality; that has to be sought for; and it must appear now somewhat of a fluke that it exists and could be found. For it is clearly conceivable that there might have been no magnitude less complicated than CMW whose relation of equality agreed with that of QM;

and if nothing simpler had been found, it would have been conceivable that the required magnitude, if it existed at all, was so complicated that it never could be found. Measurability is a fortuitous property of QM, not involved in its meaning; that is why it is better not to call QM a magnitude, or to say that it is the same magnitude * as CMW; it is better to say that CMW measures QM, and thus to leave open at this stage the question whether QM could be measured at all if it were not measured by CMW.

§4. In the discussion of the measurement of mass (or inertia), history is far less informing. Men must have recognized from a very early date that all matter resists being set into (even horizontal) motion. But the distinguishing of this property from others, and in particular from weight and quantity of matter, came very gradually, and (so far as QM is concerned) appears not to be complete even today. Yet we can see now that as soon as the property was recognized at all, a method of measuring it was available. The meaning of the property, namely resistance to being set into motion, indicates at once the appropriate relation of equality, and the relation of addition is so simple that it must have been found immediately; indeed, here the confusion with QM would have helped.

Today we might proceed thus. We use a pendulum, always starting from the same position and picking up bodies at the same point in its swing. The masses of two bodies are equal if, whichever is picked up, the end position of the swing is the same; the masses of bodies are added when they are picked up by the pendulum at the same time. By these definitions and the assignment of the numeral 1 to some standard, a standard series can be established by comparison with which the masses of all other bodies can be measured. Theory nowadays tells us that the measurement would not be "really" satisfactory, and that various corrections ought to be applied. But it would be easy to establish the method in circumstances such that all these corrections would be completely masked by irregular errors; indeed, it would be very difficult to make them experimentally significant.

Having established separately the measurement of QM and of mass, we can proceed to prove experimentally that mass is the same magnitude as CMW, and, therefore, also measures QM. Interpreted in this sense, the statement that mass is quantity of matter is a highly significant proposition, as significant and as important as the proposition that the current which charges a condenser is the same magnitude as the current which produces a magnetic field. The reason why so many foolish persons † have sneered at it is that it is not associated in our textbooks with statements, essential to its significance, of how mass and QM are to be measured independently of each other. It is for historians to decide why the originator of these statements, whoever he was, omitted the necessary explanation, or, if he did give it, why subsequent writers have not followed him. ‡

* For the meaning of "same magnitude" see *Phil. Mag.* (loc. cit.), p. 764.

† E.g. Norman Campbell, *Physics*, p. 5.

‡ Whenever I indulge in history, somebody (usually Dr. Harold Jeffreys) points out that my history is wrong. But probably here and in § 3, as in other cases, my history is near enough to the truth to serve its immediate end.

Once it has been established that mass and CMW are the same magnitude, CMW can be used as a measure of mass, having the advantage that it is more precise than any other. Indeed, if we are interested only in dynamics, and not, e.g., in chemistry, we can forget QM entirely. Its importance for dynamics is purely historical; it actually led to the use of CMW; but if ideas had developed differently, the use of CMW might have arisen independently.

§5. We now turn to force. We start by regarding it as a property of those material systems, stretched springs, solenoids, and even the human body, which (in ordinary parlance) exert force. A spring can be calibrated by addition so as to read the forces corresponding to its different extensions, as an ammeter can be calibrated to read the currents corresponding to its different deflections.* Two springs are exerting equal forces when they take up an equilibrium position in which they exert forces on each other in opposite directions; forces are added when two springs combine to stretch a third. These statements are not, of course, formal definitions; their purpose is merely to call our meaning to the minds of those versed in the arts of the laboratory; they would have to be explained to an elementary student by demonstration. When he has realized that the process, though very difficult to describe concisely, is one by which springs can be calibrated so as to make two and two equal to four, he has realized all that is necessary, indeed all that can be stated formally.

We now show that, if the spring is arranged vertically, and bodies are suspended from it, the spring is stretched to calibration marks proportional to the mass of the bodies. We thus prove that the *weight* of bodies, the force necessary to suspend them in equilibrium near the earth, is proportional to their mass; that, again, is a law directly established by experiment.

§6. We have now to establish the algebraic law written by pedagogues $P=ma$. In order to establish any algebraic law, the magnitudes involved in it must be constant during any one set of observations. We must, therefore, choose conditions in which $a$ is constant. If the time that any set of observations is to occupy exceeds a small fraction of a second, we are thereby practically confined to gravitational forces; for only these are constant over considerable distance. We could remove this limitation by using modern methods of measuring very short distances and times, but unless we could also make negligible the mass of the spring or other means for exerting force, very little would be gained.

The apparatus proposed is therefore a slider drawn along a horizontal surface with the least possible friction by a string passing over a pulley and attached to a "weight". There are several distinct stages in the experiment. In the first we establish that, whatever the mass of the slider or the weight, the motion of the slider obeys the law $x=at^2$, and is, therefore, characterized by a uniform acceleration† $a$. We then keep the weight the same and add masses $m$ to the

* See *Proc. Phys. Soc.* (loc. cit.)

† For the omission of the usual $\frac{1}{2}$ in $x=at^2$, see *Phil. Mag.* (loc. cit.), p. 767. It is usually introduced because $a$ is regarded as arising from integration of the equation $v=at$, and not from a direct relation between $x$ and $t$.

slider. Measuring $a$ and $m$, we find

$$a = C/(m+m_0), \qquad \ldots\ldots(1)$$

where $C$ and $m_0$ are constant. Comparing several experiments of this kind, we find (if the mass of the pulley is inappreciable) that $m_0$ is the mass of the weight + slider, so that (1) becomes $a = C/M$, where $M$ is the whole mass that moves with acceleration $a$. Lastly, we find by the same comparisons that $C$ is proportional to the weight of the "weight", measured as in § 5. The constant of proportionality will depend on the units chosen for mass and force, but it immediately appears that it is convenient to choose them so that the ratio of the weight of a body to its mass is the uniform acceleration $g$ that it undergoes when free. (Of course the existence of this uniform acceleration has been determined by a separate experiment.) For then (1) reduces to

$$a = m_1 g/M, \qquad \ldots\ldots(2)$$

where $m_1$ is the mass of the weight alone; and free fall becomes merely a special case of (2) in which the weight of the "weight" acts only on its own mass.

§ 7. Having obtained the clue from (1), we see at once how to generalize the law somewhat. For example, we can allow for the mass of the pulley, if that is appreciable, and we can extend the law to experiments in which the constant accelerating force is produced by springs, solenoids, etc., and not by weights. Indeed, we can go further. If we accept the Newtonian proposition,* we can extend the laws to forces that are not constant; we can show that some "kinematic forces" (e.g. air resistance), arising from motion and non-existent in a state of true equilibrium, can be measured by balancing them against static forces; and by (e.g.) connecting two bodies on a smooth table by a stretched spring we can show that the acceleration in $P = ma$ must be sometimes referred to the centre of mass. But we cannot prove the Newtonian theory, namely that it is always possible to choose a frame of reference such that any motion whatever of any number of bodies can be described by the application of equations $P = ma$, one for each pair of bodies, with equality of action and reaction. For that is a theory involving in its fullest application concepts that are not related to experiment, except on the assumption that the theory is true; thus some "kinematic forces" are *not* measurable apart from the theory. It is especially important nowadays to realize that the theory cannot be proved by such experiments as we have been considering, for the theory is now known to be false, although all the laws are true; the theory and the laws cannot, therefore, be logically connected, although it is a very difficult matter, as it always is, to detect the exact point at which generalizing laws passes into theorizing.

§ 8. The distinction between laws and theories on which I have so often insisted † is essential to this exposition. For otherwise it would be too simple; it would fail to convince because it would not show how the difficulties that it is

* See *Measurement and Calculation*, p. 208. The proposition is, roughly, that a variable derivative such as $d^2x/dt^2$ has the same physical significance as a constant derivative of the same form.

† See especially *Physics*, chap. VI.

directed to remove ever arose. The difficulties concerning mass and force, in which Mach (or perhaps his followers) have entangled us, arose from their view that theories are the ultimate propositions of science, from which laws are somewhat trivial deductions, and that the concepts involved in theories have significance apart from laws and apart from the truth of the theories they are used to state. I believe that this view is false; that, even if it were true, and if theories could be stated in terms of independently defined concepts, it would conceal the true meaning of science. I believe, on the contrary, that experimental laws are the fundamental propositions of physics, and that theories are significant only in so far as they explain laws and contain concepts that can be related directly to experiment.

This is not, of course, the place to argue the matter, but I would suggest to those who adopt the first view that it may not be desirable to present the full truth to immature minds at the start, especially if the "full truth" (namely the Newtonian theory) is not actually true; and that the alternative view, since it leads naturally to following the method of successive approximation by which science actually develops, may be tolerated as a guide in elementary education.

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# NOTE ON IMAGE FORMATION BY ROTATING MIRRORS

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*ABSTRACT.* When we view a scene through a system of mirrors, some or all of which are moving with uniform angular velocities about fixed axes, the motion of the image we see may have components of several periods. If the image moves with uniform angular velocity about a fixed axis, this velocity is twice the sum or difference of some or all of the angular velocities of the mirrors; odd multiples of these angular velocities are excluded. It follows that if we wish to bring a moving scene to rest—for instance, to project on a screen a picture recorded on a cinematograph film—by means of mirrors, it is essential to use a half-speed reflector.

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At first sight this note may appear to be an essay in proving the obvious. A novice in the study of geometrical optics would easily show that, if a mirror is rotating, an incident ray moves through an angle twice as great as that through which the mirror moves; and he might well regard it as obvious that this factor of two could not be reduced by further reflexions. But, in fact, the problem is not quite so simple. This particular result is based on the assumption that the mirror rotates about an axis in or parallel to its own plane. Other cases lead to different results. For example, a ray parallel to the axis of rotation may either suffer no deviation or rotate just as rapidly as the mirror. The properties of well-known forms of reflectors, such as pentagonal and tetragonal prisms, suggest that detailed consideration is necessary before general conclusions

can be drawn. It is important to realize that the problem is essentially three-dimensional, and that it is illegitimate to treat it by two-dimensional analysis.

For convenience we start from the law of reflexion in its analytical form. It is sometimes stated that this law can be obtained by inserting the value $-1$ for one of the refractive indices in the law of refraction.* As this is not true, a different derivation of the law, which it is thought may prove convincing, will be given. The principle employed is that if P is a real point on an incident ray which is reflected at O, the incident and reflected rays travel in the directions PO and P′O respectively, where P′ is the image of P in the mirror. It is known from the geometrical statement of the law of reflexion that PP′ is normal to the mirror (which is assumed to be plane) and is bisected by it. Take O as the origin of rectangular co-ordinates, and let $(l, m, n)$ be the direction cosines of the mirror, $(x, y, z)$ the co-ordinates of P, and $(x', y', z')$ those of P′. Since PP′ is in the direction of the normal,

$$\frac{x'-x}{l} = \frac{y'-y}{m} = \frac{z'-z}{n} = 2k, \text{ say,}$$

so that

$$\frac{x'+x}{2} = x+kl, \quad \frac{y'+y}{2} = y+km, \quad \frac{z'+z}{2} = z+kn.$$

These are the co-ordinates of a point on the mirror, the equation of which is $lX+mY+nZ=0$. It at once follows that $k+lx+my+nz=0$, and by substituting for $k$ we obtain the matrix equation

$$(x' \quad y' \quad z') = (x \quad y \quad z)r,$$

where

$$r = 1-2\begin{bmatrix} l \\ m \\ n \end{bmatrix}(l \quad m \quad n). \qquad \ldots\ldots(1)$$

Now let P be on a sphere of unit radius with O as centre. Then P′ lies on the same sphere, and if the unit vectors defining the directions of the incident and reflected rays are $u_0$ and $u_1$, $u_0 = -(x \quad y \quad z)$, $u_1 = -(x' \quad y' \quad z')$; the equation of reflexion is therefore

$$u_1 = u_0 r. \qquad \ldots\ldots(2.1)$$

If reflexion takes place at any number $p$ of mirrors in succession, repeated application of (2.1) gives for the direction of the ray as it finally emerges

$$u_p = u_0 R, \qquad \ldots\ldots(2.2)$$

where

$$R = r_1 r_2 \ldots . r_p, \qquad \ldots\ldots(3)$$

and $r_1, r_2, \ldots . r_p$ are the values of $r$ for the separate reflecting surfaces taken in order.

The rotation both of rays and of mirrors may be represented by rotating the systems of axes to which they are referred. Thus a change in the direction

* If the law of reflexion derived in this way were correct there would be no lateral inversion.

of any ray may be denoted by substituting $ut$ for $u$, where $t$ is a third-order orthogonal matrix. Similarly if a mirror is rotated $(l\ m\ n)$ will be replaced by $(l\ m\ n)t$, and correspondingly $\begin{pmatrix} l \\ m \\ n \end{pmatrix}$ by $t'\begin{pmatrix} l \\ m \\ n \end{pmatrix}$, where $t'$ is the transposed of $t$. It follows that $r$ will be replaced by $t'rt$, and if $R$ is the matrix of a group of mirrors which all undergo the same directional changes, by equation (3), in place of $R$ we shall have $t'r_1t.t'r_2t.\ldots.t'r_pt$; in virtue of the orthogonal condition $tt'=1$ this becomes $t'Rt$.

The matrix

$$t=\begin{pmatrix} 1 & . & . \\ . & \cos\theta & -\sin\theta \\ . & \sin\theta & \cos\theta \end{pmatrix},$$

which represents a rotation about the axis of $x$ through the angle $\theta$, can be expressed in the form $(-i)\,[\theta]\,(i)$, where

$$[\theta]=\begin{pmatrix} 1 & . & . \\ . & e^{\theta i} & . \\ . & . & e^{-\theta i} \end{pmatrix}, \qquad (i)=\begin{pmatrix} 1 & . & . \\ . & \sqrt{\tfrac{1}{2}} & i\sqrt{\tfrac{1}{2}} \\ . & i\sqrt{\tfrac{1}{2}} & \sqrt{\tfrac{1}{2}} \end{pmatrix}$$

and $(-i)$ is obtained from $(i)$ by changing the sign of $i$. More generally, if

$$\sigma=\begin{pmatrix} \lambda & \lambda' & \lambda'' \\ \mu & \mu' & \mu'' \\ \nu & \nu' & \nu'' \end{pmatrix},$$

where $(\lambda, \mu, \nu)$, $(\lambda', \mu', \nu')$, $(\lambda'', \mu'', \nu'')$ are the direction cosines of any three mutually perpendicular lines, the matrix

$$t=\sigma(-i)\,[\theta]\,(i)\,\sigma' \qquad \ldots\ldots(4.1)$$

denotes a rotation through the angle $\theta$ about an axis in the direction $(\lambda, \mu, \nu)$. Since both $t$ and $\sigma$ are orthogonal, we may write for the transposed of $t$

$$t'=\sigma(-i)\,[-\theta]\,(i)\sigma'. \qquad \ldots\ldots(4.2)$$

We now suppose, as we may do without loss of generality, that the whole train of mirrors is divided into groups represented by $R_1, R_2, \ldots. R_q$, of which those with odd suffixes are stationary and those with even suffixes rotate about fixed axes. In place of (2.2) we now obtain for the direction $v$ of an emergent ray the equation

$$v=u_0t_0R_1t_2'R_2t_2R_3t_4'R_4t_4\ldots.t_q'R_qt_q, \qquad \ldots\ldots(5)$$

where we have assumed, as we are at liberty to do, that $q$ is even. The rotation $t_0$ relates to the incident ray, and the significance of the rest of the notation is sufficiently obvious. By substituting from (4.1) and (4.2) this equation takes the form

$$v=u_0\sigma_0(-i)[\theta_0]S_1[-\theta_2]S_2[\theta_2]S_3[-\theta_4]\ldots.[\theta_q](i)\sigma_q'. \qquad \ldots\ldots(6)$$

The combination in which the angles $\theta_0$, $\theta_2$.... occur in this product depends only on the intervening matrices $S$. Even if all these angles change at uniform rates, it appears that the changes in $v$ will not as a rule be simple. In special cases $v$ will be independent of these angles: the image of the object whose motion is defined by $\theta_0$ is then stationary. Clearly this implies a relation of the form

$$\theta_0+\alpha\theta_2+\beta\theta_4+\ldots.=\text{constant}, \qquad \ldots\ldots(7)$$

where $\alpha$, $\beta$, .... may apparently have any of the values $-2$, $-1$, 0, 1, 2. It is easy to verify that

$$\begin{bmatrix} \pm 1 & . & . \\ . & a & . \\ . & . & b \end{bmatrix} \quad \text{and} \quad \begin{bmatrix} \pm 1 & . & . \\ . & . & c \\ . & d & . \end{bmatrix}$$

are possible forms for $S$. These make the coefficients $\alpha$, $\beta$, .... in (7) all even or zero. Several cases of this kind are well known. The only question of outstanding interest is whether odd values of these coefficients are possible. They certainly will be possible unless they are prohibited by restrictions on the form of $S$.

From equations (4.1), (4.2), (5) and (6) we note that each $S$ is of the form

$$S=(i)T(-i),$$

where $T$ is a real matrix. Using the usual suffix notation for rows and columns, this equation is

$$S=\begin{bmatrix} T_{11} & \frac{T_{12}}{\sqrt{2}} & \frac{T_{13}}{\sqrt{2}} \\ \frac{T_{21}}{\sqrt{2}} & \frac{T_{22}+T_{33}}{2} & \frac{T_{23}+T_{32}}{2} \\ \frac{T_{31}}{\sqrt{2}} & \frac{T_{23}+T_{32}}{2} & \frac{T_{22}+T_{33}}{2} \end{bmatrix} + i \begin{bmatrix} . & -\frac{T_{13}}{\sqrt{2}} & -\frac{T_{12}}{\sqrt{2}} \\ \frac{T_{31}}{\sqrt{2}} & \frac{T_{32}-T_{23}}{2} & \frac{T_{33}-T_{22}}{2} \\ \frac{T_{21}}{\sqrt{2}} & \frac{T_{22}-T_{33}}{2} & \frac{T_{23}-T_{32}}{2} \end{bmatrix} \quad \ldots.(8)$$

Consider now a product of the form $[\pm\theta]\,S\,[\mp\theta]$, say including an $S$ with an even suffix; the only way of excluding the term in $e^{2\theta i}$ is to make $S_{32}=0$, when (8) shows that $e^{-2\theta i}$ is also excluded identically. The condition that $2\theta$ is absent is thus that $T$ is of the form

$$T=\begin{bmatrix} T_{11} & T_{12} & T_{13} \\ T_{21} & T_{22} & T_{23} \\ T_{31} & -T_{23} & T_{22} \end{bmatrix}.$$

Now from (1) $r$ is orthogonal, and so are all the other factors in $T$: hence $T$ itself is orthogonal. The condition $(TT')_{23}=0$ is the same as $T_{21}T_{31}=0$, and since $(TT')_{22}=1=(TT')_{33}$, we have ${T_{21}}^2={T_{31}}^2$; therefore both $T_{21}$ and $T_{31}$ vanish, and ${T_{22}}^2+{T_{23}}^2=1$. Also $0=(TT')_{21}{}^2+(TT')_{31}{}^2=({T_{12}}^2+{T_{13}}^2)({T_{22}}^2+{T_{23}}^2)$, or $T_{12}=T_{13}=0$. We now note from (8) that these conditions referring to the elements of $T$ imply that $S$ is a diagonal matrix. In other words, the product $[-\theta]\,S\,[\theta]$ either contains the terms $e^{2\theta i}$ and $e^{-2\theta i}$ or is independent of $\theta$.

We may clearly exclude the latter alternative, for it implies that the rotation of this group of mirrors produces no effect. Our only resource for eliminating the even multiples of $\theta$ then lies in choosing suitable forms for $S_3$, $S_5$, ..... It is readily seen that the suppression of the numerically greatest term in $2(\pm\theta_2 \pm \theta_4 \ldots.)$ only leaves two cases for consideration. One corresponds exactly to the case already considered, and fails to give a solution of the kind required. In the other, $S_{22}$ and $S_{33}$ are zero, and this again leads, by a similar argument to that already used, to $S_{12}=S_{13}=S_{21}=S_{31}=0$. These, however, are the coefficients of the only terms containing odd multiples of $\theta$. It follows that a moving scene cannot be brought to rest by reflexion at mirrors without employing angular velocities which are even submultiples of that of the object.

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# LABORATORY EXPERIMENTS ON THE MAGNETIZATION OF ROCKS‡

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*ABSTRACT.* Specimens of natural rock masses collected by H.M. Geological Survey were tested for magnetic susceptibility and permanent magnetization before and after various artificial treatments. After cooling in the earth's field, cut cubes were found to be magnetized with an intensity much greater than that of the natural rock, and the values decayed very little with time. When artificially magnetized in the cold, the cubes were only affected by fields above a certain value, and the decay was often considerable. Curves are given showing the decay with time and the demagnetization of the heated cubes by increasing fields. The susceptibility of natural rocks increases with the field, in some cases reaching a maximum between 50 and 100 C.G.S. These results are compared with data by J. G. Koenigsberger.

## §1. INTRODUCTION

MAGNETIC data for several natural rock masses have already been recorded (Hallimond and Herroun, 1933). The measurements now to be described were made some years ago in pursuance of this work; they relate chiefly to the same rocks when subjected to various artificial treatments. The magnetic behaviour of these rocks is complex, and many further investigations could be devised; but the present series of experiments may be of use as an indication of the general nature of the problems encountered, and the precautions that must be observed. Many detailed measurements, chiefly upon foreign rocks, have been described by J. G. Koenigsberger (1931–8: see especially 1935 and 1938); the present experiments deal with British specimens collected by

‡ Published by permission of the Director, Geological Survey and Museum.

H.M. Geological Survey, in several cases from rock masses over which field-magnetometer surveys have been made. Many of the observed magnetic anomalies can be satisfactorily explained if it is assumed that the rock possesses a uniform magnetization which is in part induced by the earth's present field and in part due to a permanent magnetization acquired at some earlier period. Whatever conclusions may ultimately be reached as to the source of the latter, a fuller knowledge of the magnetic state of the natural rocks is essential for the interpretation of geophysical surveys: it may also be of interest in its bearing on geological age and on the magnetic history of the earth.

*Apparatus.* With small modifications this was the same as that previously described. Susceptibilities were also measured upon cut bars ($1 \times 3 \cdot 5$ cm.) and powdered rock in the Wilson (1919, 1920) magnetic balance.

*List of rocks used*

Olivine basalt (Roadstone Coll. No. 970), Seacliff Quarry, N. Berwick (depth unknown).

Dolerite, Cleveland Dyke, Goathland, N. Yorks. (depth about 100 ft.).

Tholeite Dyke, Cats Craig Quarry, Lornty, Blairgowrie, Perthshire (depth about 30 ft.).

Greenstone Dyke, Mt. Sorrel Granite Quarry, Leics. (depth about 30 ft.).

Granophyre, Bradgate Hill Quarry, Groby, Leics. (depth about 20 ft.).

Granite, Mt. Sorrel, Leics. Grey variety. (Spec. L 15, Wilson, 1919.)

## § 2. PERMANENT MAGNETIZATION OF ROCKS BY COOLING IN THE EARTH'S PRESENT FIELD

It is generally accepted that permanent magnetization is destroyed by heating to red heat (except a small contribution from haematite, etc.: see Koenisgberger, 1935). On cooling, the rock becomes magnetized in the direction of the applied field at the time; it is convenient, therefore, to ascertain the degree of magnetization imparted to a rock by cooling in the earth's field. As far as is known at present, this seems one of the two obvious means by which the igneous rocks could have acquired a permanent magnetization throughout their bulk; the other is the application of a rather strong general field at some later period.

The extent to which the present rocks can be magnetized by cooling in the earth's field was tested by cooling the specimens in an electric muffle and determining their magnetization afterwards on the magnetometer. Cubes of 1½-inch side were marked to identify the axes and placed in an earthenware pot with their upper surfaces level and one edge of each pointing along the magnetic meridian; the pot was filled with powdered iron ore to hinder the oxidation of the cubes, and was placed in an upright furnace wound with nichrome wire and built without any iron fittings. After heating for about an hour at a red heat, the current was cut off and the specimens cooled in the furnace. The magnetization was then determined along A, B and C, the three axes of the cube; the results are shown in table 1.

*Alteration by heating.* The iron carbonate surrounding the cubes was partially oxidized during the heating; it was used in order to produce conditions comparable with those within the rock, and it was at least partially effective in preventing the access of oxygen from outside. Unfortunately a second source of oxygen is present, in the steam, etc., which all rocks give off to some extent at a red heat. This cannot be excluded, and the magnetic state of the rocks after heating was generally found to be different from their original condition. For the two rocks from Leicestershire there was an increase of susceptibility, probably due to the re-formation of magnetite in rocks that had been oxidized under desert conditions; two fresh dyke rocks show a diminution. Alteration

Table 1. Magnetization of cut cubes by cooling in the earth's field

The numbers give the intensity of magnetization $\mathcal{J}$ along the respective axis in $10^{-5}$ C.G.S. Susceptibilities are also in $10^{-5}$ C.G.S.

Details of the rocks used have been given in a separate list above.

| Rock | Seacliff basalt | | Cleveland dolerite | | Mt. Sorrel greenstone | | Bradgate granophyre | | Lornty tholeite | |
|---|---|---|---|---|---|---|---|---|---|---|
| Cube axis | as cut | cooled | as cut | cooled | as cut | cooled | as cut | cooled | as cut | cooled |
| AA′ | 121 | 207 | 22 | 163 | 0 | 321 | 0 | 196 | −6 | 140 |
| BB′ | 26 | 0 | 37 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| CC′ | 69 | 519 | 0 | 409 | 0 | 750 | 0 | 482 | 26·5 | 348 |
| Ratio C : A | | 2·5 | | 2·5 | | 2·3 | | 2·5 | | 2·5 |
| Susceptibility: | | | | | | | | | | |
| Cube $H=5$ | | 174 | | 142 | | 492 | | 256 | 164 | not |
| Powder $H=13·5$ | 363 | | | | 36·5 | | | 265 | | deter- |
| Cut bar do. | 387 | | 253 | | | | 5·4[1] | | | mined |
| $\mathcal{J}$ | 144 | 558 | 43 | 440 | 0 | 816 | 0 | 520 | 27 | 375 |
| $Q=\mathcal{J}/\mathcal{J}_k$ | 0·9 | 6·4 | 0·17 | 6·2 | 0 | 3·30 | 0 | 4·06 | 0·32 | |

[1] In field $H=140$.

The first four rocks were cut in arbitrary directions; Lornty was cut from a piece of rock oriented with the axes C vertical, A magnetic N.–S., B magnetic E.–W. when *in situ*.

$Q$ (Koenigsberger) is the ratio of the total permanent magnetization to that induced in the same rock by the earth's present field (0·5 C.G.S.).

For these low susceptibilities the demagnetizing factor seems unlikely to have a serious effect on the determinations.

seems inevitable, but the altered rocks are probably still reasonably similar to natural rocks, so that the results will indicate the general degree of magnetization that may be expected in a rock after cooling in a field similar to that now due to the earth. No precise value can be laid down, for the properties of magnetite vary considerably. The ratio of the magnetization to the susceptibility *after* heating agrees with the values found by Koenigsberger (1935) for his ratio $Q$, but it is unfortunately not applicable numerically to the rock in its original state; still the ratios for the unheated rocks are very much smaller, in agreement with Koenigsberger's values, which led him to the view that part of their initial magnetization has been lost.

*Decay with time.* In contrast with rocks magnetized in the cold, the values decay very little with the passage of time. The measurements in table 1 were made usually 24 to 36 hours after cooling; Seacliff was re-measured four months after cooling and gave substantially the same readings as before.

## §3. ARTIFICIAL MAGNETIZATION IN THE COLD

Generally speaking, the field required to magnetize a rock in the cold is greater than that needed on cooling through the critical range. Caution is necessary in devising these tests, for the results are found to vary with the previous magnetic history of the specimen and with the lapse of time. The present experiments are far from comprehensive, but they are recorded since they bear directly upon the question whether the natural magnetization of rocks was acquired during cooling or at some later period under the influence of abnormal magnetic fields.

*Relation to field strength.* Tests of the residual magnetization were made in several cases by switching off the magnetizing current while a cube was in the solenoid. With weak fields there was no permanent magnetization. Thus Lornty after field 5 C.G.S. gave no appreciable reading. A cube of Cleveland Dyke was measured a few hours after magnetization; it gave for $\mathfrak{J}$ after 1 C.G.S. nil; 5 C.G.S. 73; 10 C.G.S. 328; 25 C.G.S. about 700 ($\times 10^{-6}$). It would seem that fields no greater than that of the earth will produce no permanent magnetization, but that above a certain value, which may vary for different rocks, permanent magnetization sets in and increases rapidly with the field strength. Samples of magnetic rocks can thus be moved safely, provided they are at no time exposed to a field greater than that of the earth.

Much higher magnetization can be produced by large fields. Thus Seacliff, which gave in field 88 C.G.S. $\mathfrak{J}=0{\cdot}142$, when subjected to $H=1000$ to 1200 gave (after 3 hours) $\mathfrak{J}=0{\cdot}259$ C.G.S. In general form the magnetization curve of the rocks resembles that of iron (cf. Pockels, 1897).

*Successive magnetization along the cube axes.* It might perhaps be supposed that for moderate values the magnetization resulting from the application of a second field at right angles to the first would take place independently, so that the final value would be that obtained by compounding the two effects. Experiment shows that this is very far from being the case. A cube of Seacliff was magnetized in a field of 88 C.G.S., applied successively along the principal axes A, B, C. Three hours after each magnetization the components were determined, with the following result:

| | Intensity of magnetization along the axis | | |
|---|---|---|---|
| Magnetized along | A | B | C |
| A | 0·142 | — | — |
| B | 0·054 | 0·1356 | — |
| C | 0·022 | not determined | 0·134 |

Thus, although the second and third directions were at right angles to the first, the intensity along A was diminished by each magnetization to less than half its previous value The intensity produced along the direction of the applied field was, however, nearly the same whether the cube had been previously magnetized or not.

*Decay of magnetization with time.* The cube of Lornty, after the third heating, was demagnetized by alternating current and then magnetized along its original N.S. axis, but in the reverse direction, in a field of 88 C.G.S. It was placed on the turntable of the magnetometer and deflection on reversal measured at times of 1, 2, 5 minutes, etc. It was then demagnetized again and remagnetized by a field of 56 C.G.S. along its original E.W. axis, and a second series of readings was taken. The two decay curves are plotted in figure 1, in which

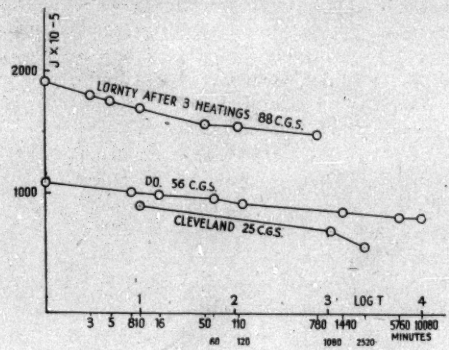


Figure 1. Decay of magnetization with time. Specimens magnetized at room temperature.

the abscissa is $\log t$. There is a steady but diminishing loss of magnetism; both curves suggest that the decay becomes very slow after more than 96 hours, at which time the loss represents about 1/3 of the value at 1 minute. For comparison, the third curve gives the decay for a cube of unheated rock (Cleveland) after magnetization in field 25 C.G.S. Its course is similar to that for the heated rock in field 56 C.G.S. Evidently it is necessary to specify the times in discussing the magnetization of rocks in the cold.

## §4. DEMAGNETIZATION BY APPLIED FIELDS

Four cubes that had been magnetized by cooling in the earth's field were demagnetized by applying fields of increasing strength along the vertical axis of the cube, in a solenoid. They yielded the curves shown in figure 2. The readings were taken immediately after the removal of the demagnetizing field and, as might be expected for a magnetization in the cold, there is a marked recovery on standing. Thus Lornty (first heating), after application of field 25 C.G.S., was allowed to stand 24 hours and the value rose by 164 to 255, but on re-applying the same field it regained its former value and the curve continued. After a stronger field, the recovery was less, being only 55 when the magnetization had been brought to zero by application of field 34 C.G.S. Evidently the effect of the reverse field is complex; the relatively permanent initial magnetization is in part destroyed, and there is a fugitive reverse magnetization produced in the cold.

As a group, the curves indicate that the field required to demagnetize completely is roughly proportional to the original magnetization. This would be against expectation, if the intensity merely depended upon the percentage of sparse, similar, equally magnetized particles, since the same field should suffice to demagnetize them in all the rocks, whatever their number. If, on the other hand, the higher values of $\mathfrak{J}$ are due to higher magnetization of the particles themselves, although the field used is the same, it would follow that the magnetic properties must have changed with the treatment, e.g. by the varying degree of oxidation in a solid solution: magnetite itself is often very stable when heated (Wilson and Herroun, 1919). Natural rocks certainly depend to some extent on the number of particles for their magnetic values; they cannot therefore be expected

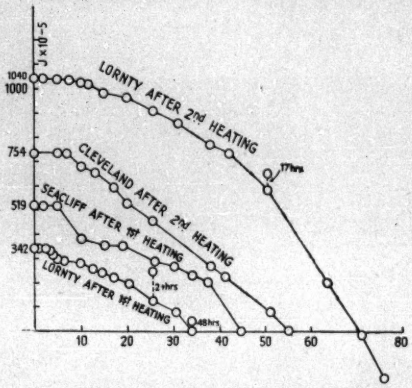


Figure 2. Demagnetization of cooled cubes by increasing fields.

to give curves with the same proportionality as those in figure 2; indeed, the least similar rock, Seacliff, shows a rather distinctive curve even after it has been heated like the others.

The demagnetization of Seacliff along the vertical axis (above) resulted in only a slight diminution of the magnetization along the N.S. axis, in keeping with the relatively stable nature of magnetization produced by cooling. This was fully confirmed in the case of Lornty (second heating), which was demagnetized by a field of over 70 C.G.S. and yet still gave the initial reading along the N.S. axis. These results offer a striking contrast to those for cubes magnetized n the cold, in which much of the cross-magnetization was destroyed.

As was found for magnetization in the cold, there is no demagnetizing effect until the applied field exceeds a value of 2–5 C.G.S.

## § 5. COMPARISON BETWEEN MAGNETIZATION BY COOLING AND BY APPLIED FIELDS

Natural rocks sometimes have permanent magnetization of the order of 100 to $500 \times 10^{-5}$ C.G.S., comparable with the values produced in a cube on cooling in the earth's present field (table 1). To produce a similar magnetization

in the cold, the rock must be exposed to a field of 5 to 15 C.G.S., and allowing for the subsequent decay twice these values might well be required, i.e. from 10 to 30 times the earth's field (cf. similar results for magnetite, Wilson and Herroun, 1919, p. 315). In the absence of other evidence for such fields the data favour the view that cooling is a sufficient cause. Further, the large field, if external, must be applied for only a brief interval, since the earth's diurnal rotation would cause any field exceeding the minimum to exert a demagnetizing action, which would partially destroy any existing magnetization. Koenigsberger finds that the magnetization of the older rocks is usually much lower than would be expected from their susceptibility, if the earth's field when they cooled were the same as today (cf. table 1): either a weaker field or a subsequent demagnetization seems to be indicated, even when allowance is made for possible movement in the rock while cooling through the Curie point.

## § 6. VARIATION OF MAGNETIC SUSCEPTIBILITY WITH FIELD-STRENGTH

Magnetic anomalies, as measured in magnetometer surveys, are due in part to magnetization of the rock masses in the earth's field, of which the vertical component is about 0·4 C.G.S. Most laboratory determinations are made with larger fields, up to 100 C.G.S., within which range the susceptibility of a rock increases considerably with the field strength (cf. Koenigsberger, 1931).

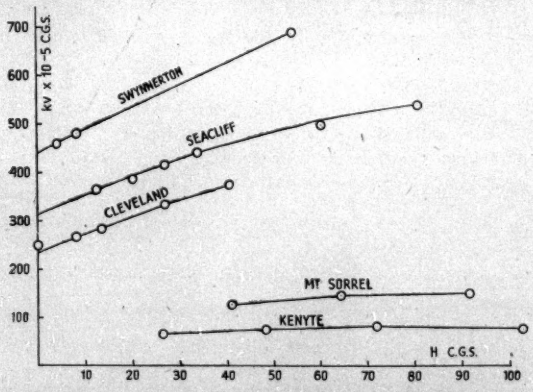


Figure 3. Increase in susceptibility with increasing fields.

Early observations were made on the Mount Sorrel granite (L15, Wilson, 1919) and on the rock kenyte (Wilson, 1920, p. 283); these are shown in figure 3, the other curves being from unpublished determinations made by the late Professor E. Wilson. There is a regular increase with field strength, the values indicated for very low fields being Swynnerton 430, Seacliff 310, Cleveland 230 ($\times 10^{-6}$). These are in accord with the values Swynnerton 400, deduced from the field survey at another locality, and Cleveland 250, by direct measurements on a cube in field 1 C.G.S. Comparison of data obtained from different

samples must, however, be made with reserve, for small pieces from the same rock may very considerably in magnetic properties.

As already noted, fields above about 5 C.G.S. cause permanent magnetization of the rock. Care was taken that the field used did not previously exceed the value at which each determination was made; the results are therefore comparable, but a part of the force obtained with the higher fields is due to permanent magnetization, which is included in the susceptibility. For fields comparable with that of the earth the whole of the effect is temporary and can be treated separately from the permanent magnetization. The agreement obtained between $Kv$ deduced from magnetic surveys and the laboratory values indicates that there is little or no building up of magnetization during prolonged exposure to the earth's magnetic field.

The susceptibility of crystalline magnetite has been investigated by Wilson and Herroun (1919). It usually increases with the applied field between $H=0$ and 200, but for certain pure samples a sharp maximum was reached at $H=20$ to 50. There is no sign of the latter in the present curves. The curve for kenyte (figure 3) has a blunt maximum about $H=70$, and that for Mount Sorrel is rather similar; these would agree with the intermediate types of Wilson and Herroun. Grain size is no doubt an important factor, as well as variation in the magnetic mineral.

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# DISCUSSION

Mr. Awbery. As instancing the complexity of these phenomena, the table in § 3 is interesting. From the first column we see that a component $J$ of 0·054 along the A-axis is reduced to 0·022 by magnetizing along the C-axis. We should thus expect the $J$-value of 0·136 along the B-axis to become 0·053 when magnetization along the C-axis occurs ; unfortunately it is not measured. As stated in the text, the intensity along the direction of magnetization is nearly constant (mean 0·137, with mean deviation 0·003, i.e. 2·3 %). Assuming, however, that the second column gives the result suggested above, it is noteworthy that the quantity most nearly constant is the resultant magnetization, which for the three rows of the table will be 0·142, 0·146, and 0·146 (mean 0·145, with mean deviation 0·002, i.e. 1·2 %).

Authors' reply. The results are certainly complex, and it may well be that constancy of the total magnetization will more nearly express the observed values. But further work is desirable, and it would be of interest to have observations on materials more suitable than the natural rocks, which are, unfortunately, a mixture of various minerals whose constants cannot be exactly ascertained.

# A THEORY OF THE IRREVERSIBLE ELECTRICAL RESISTANCE CHANGES OF METALLIC FILMS EVAPORATED IN VACUUM

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*Communicated* 16 *November* 1942 *by J. H. Awbery*

*ABSTRACT.* Metallic films prepared by evaporation show characteristic irreversible changes, especially if prepared at low temperatures. The changes can be explained by a decay of lattice distortions. To allow of a mathematical treatment, a set of simplifying assumptions is formulated. To initiate a decomposition of a distortion, a certain energy must be reached, but the observed changes cannot be explained if all distortions have the same decay energy. A characteristic function $F_0$ expressing the law of distribution of the decay energies is formulated, calculated from measurements available and found to be of the expected order. The shape of this function and the position of its maxima depend on the structure of the support and on the thickness of the film. The resistance measurement at a constant temperature has the disadvantage that it reveals only a small part of $F_0$. This is overcome if uniformly rising temperature is used. A possibility that the changes are caused by a recombination of interstitial atoms and lattice vacancies is investigated, but as this requires essentially only a single value of the transition energy, the results are inconsistent with the observed changes, so that the decay theory should be preferred, as it gives more consistent results.

## §1. INTRODUCTION

It is well known that metallic films deposited by the method of evaporation usually show higher specific resistance than the metal in bulk, and that their initial resistance decreases irreversibly if their temperature is uniformly raised above that at which they were prepared. If the film is cooled or heated below a temperature previously reached, only reversible changes usually result.

Moreover, the phenomenon is complicated by spontaneous decrease of resistance with time even if the film is held at a constant temperature, especially if it is above the highest temperature to which it had previously been exposed (Vand, 1936; Mitchell, 1938, and others). This decrease consists of an initial rapid fall followed by a slow asymptotic fall of long duration, as represented in figure 1. The rapid fall can be renewed by bringing the film to a new higher constant temperature. The initial resistance is much higher for films prepared at very low temperatures than at room temperature, and in either case it approaches the resistance of the metal in bulk if the film is heated near to its melting point or kept at constant temperature for a sufficiently long period of time.

These phenomena are not limited to films prepared by evaporation. Sputtered films show a similar behaviour (Pogány, 1916; Perucca, 1930–38;

Maslakovec, 1934; Braunsfurth, 1931). They are not so suitable for our study owing to the comparatively long time needed for their preparation and to their probable gas content, which complicates their behaviour, compared with evaporated films in high vacuum, which can be prepared gas free and in a short time, thus rendering possible the study of rapid resistance changes immediately after their preparation. Nevertheless, the study of sputtered films may prove very interesting as a separate subject of investigation. Cold-worked and quenched metals show similar resistance changes, but on a smaller scale than evaporated and sputtered films, and a detailed investigation of their resistance changes on similar lines may throw further light on their structure and prove useful in technology.

Several theories have been proposed to explain the observed resistance changes (Perucca, 1930–38; Reinders and Hamburger, 1931, and others). As most of them are applicable to special cases only, we shall not discuss all of them in detail. In comparatively thick films of regular metals, the changes seem most probably to be caused by a gradual approach to order in the crystal lattice. During condensation the atoms lose most of their kinetic energy by inelastic impact, and if the support is at a low temperature, they have insufficient mobility left to form a distortion-free lattice. They find the nearest potential hole on the surface either of the support or of the metal already condensed, but this position may not correspond to their place in a normal lattice. In certain circumstances the atoms will even form a lattice of an unusual type, as has several times been observed by x-ray and electron-diffraction methods (see, for example, Conference on the Conduction of Electricity in Solids at Bristol, 1937). High sensitivity of the resistance variations to the contamination of the supporting surface by a gas film or other impurity is thus to be expected, and it will be easy to understand why very different results are often obtained under apparently identical conditions.

## §2. GENERAL THEORY

It is well known that a periodic lattice field of a perfect crystal of metal at 0° K. offers no resistance to the movement of the conducting electrons (see, for example, Mott and Jones, 1936; Wilson, 1936), and that the resistance of metals is caused by scattering of conducting electrons by deviations of the lattice field from perfect periodicity. Experimental results and theoretical considerations show that the specific resistance $R$ of most metals is composed additively of two parts (*Matthiessen's rule*), viz.:

$$R = R_T + R_i, \qquad \ldots\ldots (1)$$

where $R_T$ is caused by scattering on the thermal oscillations of the lattice and is approximately proportional to the absolute temperature, and $R_i$, called ***residual resistance***, is caused by scattering on impurities and other lattice distortions, is approximately temperature independent and is equal to zero for pure perfect crystals of metal.

Differentiating equation (1) with respect to the temperature $T$, we obtain

$$dR/dT = dR_T/dT + dR_i/dT,$$

where $dR_T/dT$ is approximately constant in a wide range of temperatures and

independent of $R_i$, being thus an important characteristic of a metal. It should be pointed out that the so-called *temperature coefficient* $\alpha$ of resistance, defined by

$$\alpha = \frac{1}{R} \cdot \frac{dR}{dT},$$

is not a characteristic of a metal, being even in the reversible region a relatively complicated function of $R_i$, and is of little value for our purposes.

The part $dR_i/dT$ depends on the previous history of the film, and in the reversible region below the temperature previously reached is $dR_i/dT = 0$, and thus $dR_T/dT$ can be directly measured. These facts can be used for the separation of $R_i$ from $R_T$ with advantage by measuring $dR_T/dT$ of the specimen, preferably at the end of the measurement of the irreversible changes, and by using the formula

$$R_i = R - T \, . \, dR_T/dT.$$

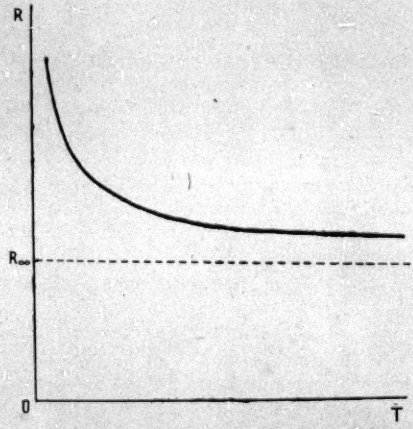


Figure 1. Typical resistance changes of a film as a function of time at constant temperature.

In addition, $dR_T/dT$ being the same as for the metal in bulk, it can be used for approximate calculation of the thickness of the specimen measured, if other means are not available.

Typical resistance changes of a film prepared at very low temperature, plotted as a function of temperature $T$, are schematically represented in figure 2. Point A represents the initial resistance of the film immediately after its deposition. If the film is uniformly heated above the temperature of its preparation, its resistance changes irreversibly along the curve ABCD, until point E is reached, where the film begins to re-evaporate or to melt, measurements above which are useless for our purposes. If the film is then cooled below the temperature of the point E, the resistance changes reversibly along the straight line EFG, and if the decomposition of all distortions was finished at the point D before E, the metal was pure and the film was not very thin, it

represents the pure part $R_T$ and accordingly vanishes at 0° K. If the film is cooled before reaching the point D, the resistance changes along a straight line parallel to EFG, one such line being shown dotted.

The decay of the distortions is often negligible at very low temperatures, and then a portion AB of the curve is also parallel to EFG. Some workers (Kramer and Zahn, 1932, 1933; Kramer, 1934, 1937; Çelebi, 1937) call the temperature of the point B, where the irreversible change actually begins, a *transition temperature*, suggesting a transition of the metal from amorphous to its normal state. For some films, particularly if deposited on a well outgassed support (Fukuroi, 1938), the point B is very ill defined, and according to our opinion the temperature of the inflexion point C, where the changes are steepest, should rather be noted as the most significant and best defined point of the curve.

Some curves show deviations from the schematic curve shown. Sometimes more inflexion points are observed, but care must be taken to distinguish them

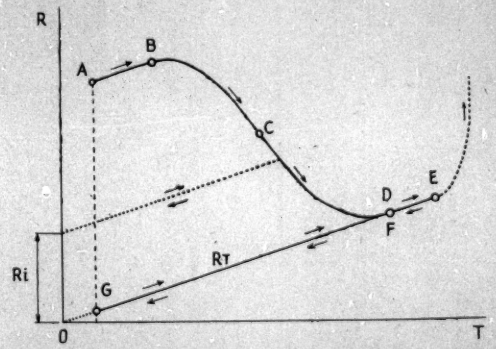


Figure 2. Typical irreversible and reversible resistance changes of a film prepared at very low temperature as a function of temperature.

from irregularities caused by uneven rise of temperature or other disturbing cause. Often, one or more parts of the curve are missing, for example part AB or DE, which may be caused by the small range of temperature used.

Some films of anomalous metals, as of bismuth and antimony, and some very thin films prepared under special conditions (Suhrmann and Berndt, 1940), show anomalous behaviour, if they are cooled in various stages before reaching the point D. The reversible resistance changes are then not parallel with each other, indicating that $dR_T/dT$ is no longer a constant independent of $R_i$. This can be attributed to a change in the number of free conducting electrons, but as these films show different behaviour from thicker films of regular metals, to which attention will be restricted here, we shall exclude them from our study.

An expression of the initial resistance of the point A in convenient units may prove useful. Some authors state how many times the resistance $R$ of the point A is higher than $R_T$ of a normal metal *at the same temperature*, represented

by the point G on figure 1. Very high ratios (of the order of 50) have been recorded. Unfortunately this ratio gives but a very poor idea of the initial degree of disorder of the film, as at low temperatures it depends inversely on the temperature of preparation, and at 0° K. this ratio is $\infty$ for any film. It appears to us that a better method is to compare the initial specific residual resistance $R_i$ with the resistance $R_T$ of a normal metal at 0° C., which will be called $R_{273}$. A still better method is to compare the initial disorder of the film to that of the liquid metal at its melting point, the specific resistance of which will be called $R_{liq.}$ (see table 1). It should be pointed out that the atoms of a liquid metal still have a comparatively regular arrangement, resembling liquid crystals to a certain degree, and that a still higher degree of disorder is thus theoretically possible; they represent, nevertheless, a convenient standard of

Table 1. Characteristic constants of some metals

| Metal | Debye frequency $f \cdot 10^{-10}$ $sec.^{-1}$ | $E_{bind.}$ Binding energy $\times 10^{14}$ erg at 0° K. | $\frac{T_{melt.}}{T_{boil.}}$ at 1 atm. | $R_{273}$. Spec. resistance of solid at 0° C., ohm-cm. $\times 10^6$ | $R_{liq.}$ Spec. resistance of liquid at melting point, ohm-cm. $\times 10^6$ | $E_M$. Max. value of $E \cdot 10^{14}$ erg from table 2 | $E_{bind.} \cdot \frac{T_{melt.}}{T_{boil.}}$ $\times 10^{14}$ erg. |
|---|---|---|---|---|---|---|---|
| Mg | 600 | 285 | 0·68 | 4·00 | — | 102 | 196 |
| Zn | 525 | 225 | 0·58 | 5·53 | 37 | 123 | 130 |
| Cd | 360 | 195 | 0·59 | 6·66 | 34 | 117 | 115 |
| Cu | 700 | 525 | 0·51 | 1·55 | 21 | 148 | 267 |
| Ag | 450 | 445 | 0·51 | 1·50 | 17 | 205 | 226 |
| Au | 355 | 574 | 0·51 | 2·04 | 30 | 227 | 293 |
| Tl | 209 | 304 | 0·33 | 14·10 | — | 96 | 100 |
| Pb | 184 | 352 | 0·30 | 19·20 | 98 | 104 | 106 |
| Hg | 140 | 128 | 0·37 | 22·8 | 90 | 36 | 47 |

disorder suitable to serve as a unit. To avoid decimal points, percentage expression is convenient, and we thus suggest, for expressing the disorder $\delta$, the following relation:

$$\delta = 100 \cdot R_i/R_{liq.} \%.$$

The values $\delta$ so obtained are found to lie between 4 % and 150 % (see table 2), being much smaller for films prepared at room temperature than for those prepared near 0° K.

As a contribution to the residual resistance of very thin films is made by the limitation of the free path of the electrons by the dimensions of the conductor (Thomson, 1901), this part is to be subtracted from $R_i$ in the relation for $\delta$, but for relatively thick films no correction is necessary.

We shall now turn our attention to changes caused by the variations of the number and quality of the lattice distortions only.

Table 2. Review of published resistance measurements of films prepared at low temperatures

| Film | Support. Outgassing temperature | Reference | Temp. of prep., °K. | Thickness, mμ | Initial resistance, ohm | $E_M$. Maxima of $F_0(E)$ observed $\times 10^{14}$ erg | Initial disorder % of liquid metal, $\delta$ |
|---|---|---|---|---|---|---|---|
| Zn | Quartz polished | (1) | 120 | — | 3050 | 108 | |
| ,, | ,, ,, | ,, | ,, | — | 400 | 110 | |
| ,, | ,, ,, | ,, | ,, | — | 1900 | 100 | |
| | | | | | | 112 | |
| ,, | ,, ,, | ,, | ,, | — | 850 | 112 | |
| ,, | Zn | ,, | ,, | — | 390 | 86 | |
| ,, | Cd | ,, | ,, | — | 250 | 80 | |
| ,, | Cd | ,, | ,, | — | 170 | 80 | |
| ,, | Ag | ,, | ,, | — | 1400 | 110 | |
| ,, | Ag | ,, | ,, | — | 240 | 105 | |
| Cd | Quartz polished | ,, | ,, | — | 1100 | 95 | |
| ,, | ,, ,, | ,, | ,, | — | 2550 | 105 | |
| Ag | Quartz | (2) | 20 | 20 | 5·05 | 100 | 100 |
| Cu | Quartz over 100° c. | (3) | 20 | 115 | 3·95 | 148 | 104 |
| Au I | ,, ,, | ,, | 80 | 80 | 3·8 | 95 | 72 |
| | | | | | | 131, | |
| | | | | | | 227 | |
| Cd | ,, ,, | ,, | ,, | 138 | 2·95 | 65 | 150 |
| | | | | | | 117 | |
| Tl | ,, ,, | ,, | ,, | 108 | 3·95 | 96 | 18 |
| Pb I | ,, ,, | ,, | ,, | 124 | 3·9 | 65 | 20 |
| | | | | | | 104 | |
| Pb II | ,, ,, | ,, | ,, | 137 | 3·75 | 88 | 15 |
| Au II | ,, ,, | ,, | 320 | — | 0·96 | 205 | 4 |
| Ag II | ,, ,, | ,, | 300 | 58 | 0·94 | 205 | 4 |
| Zn | Glass not baked | (4) | 110 | 70 | 100 | 123 | |
| ,, | ,, ,, | ,, | ,, | 110 | 25 | 110 | |
| ,, | ,, ,, | ,, | ,, | 200 | 8 | 92 | |
| Cd | ,, ,, | ,, | ,, | 50 | 128 | 112 | |
| ,, | ,, ,, | ,, | ,, | 80 | 85 | 105 | |
| ,, | ,, ,, | ,, | ,, | 195 | 8 | 85 | |
| Hg | ,, ,, | ,, | 20 to 70 | — | 3000 | 36 | |
| ,, | ,, ,, | ,, | ,, | — | 1500 | 28 | |
| Mg | ,, ,, | ,, | 120 | 200 | 90 | 102 | |
| ,, | ,, ,, | ,, | ,, | 225 | 56 | 96 | |
| ,, | ,, ,, | ,, | ,, | 275 | 16 | 88 | |
| Pb | ,, ,, | ,, | 100 | 50 | 600 | 100 | |
| ,, | ,, ,, | ,, | ,, | 90 | 160 | 98 | |
| Pb | Pyrex 250° c. | (5) | 110 | — | 560 | 100 | |
| ,, | ,, 400° c. | ,, | ,, | — | ,, | 77 | |
| ,, | Air readmitted | ,, | ,, | — | ,, | 98 | |
| ,, | Pyrex 500° c. | ,, | ,, | — | ,, | 77 | |
| Cd | ,, 250° c. | ,, | 80 | — | 600 | 90 | |
| ,, | ,, 400° c. | ,, | ,, | — | ,, | 86 | |
| ,, | ,, 500° c. | ,, | ,, | — | ,, | 86 | |
| Zn | ,, 250° c. | ,, | ,, | — | ,, | 88 | |
| ,, | ,, 400° c. | ,, | ,, | — | ,, | 85 | |
| ,, | ,, 500° c. | ,, | ,, | — | ,, | 85 | |

*Remarks to table* 2 (*see previous page*)

In the first and second columns are stated the metal deposited, the supporting material and the outgassing temperature, if the support was baked before film deposition.

References of the third column are: (1) Mitchell, 1938; (2) Suhrmann and Barth, 1934; (3) Suhrmann and Barth, 1936; (4) Fukuroi, 1937; (5) Fukuroi, 1938.

In the fourth column the temperature of the support during the deposition of the film is given. The fifth column contains the thickness of the film in mμ. If it is not given, it can be judged approximately from the initial resistance of the film, given in the sixth column. The initial resistance is not reduced to represent specific resistance, but in ref. (4) it is reduced to that of a film 1 cm. wide and 1 cm. long.

The seventh column gives the position $E_M$ of the maxima of $F_0(E)$, calculated by the method described below. If several maxima have been observed, several values of $E_M$ are given, but the material is insufficient to decide if they are all real or caused by uneven rise of temperature. In calculation for the constant $c$, equation (3), *Debye* frequency $f$, given in table 1, has been used, and $n$ taken as 10. The value of the constant $a=1$ to 1·5°/min. is given by Fukuroi for his measurements. In other cases $a=0{\cdot}01°$/sec. was used, where not stated, as a most probable estimate. The eighth column gives the initial degree of disorder $\delta$, compared to that of the liquid metal at its melting point.*

The distortions can be classified as follows:

(1) *Lattice deficiency* or *hollow type*, as lattice vacancies (Shottky defects), cavities and cracks of the most varied type.

(2) *Lattice excess* or *compressed type*, as interstitial atoms, or atoms in positions between the normal lattice points.

(3) *Combined type*, where deficiencies of the lattice are closely interlinked with the lattice compressions. A disorder on the imperfectly fitting surfaces of the adjacent crystals, and very small crystals embedded between bigger ones, probably belongs to this type of distortions.

The scattering power of a distortion for the conducting electrons is proportional to the square of deficiency or excess of its electrical charge as compared with the average charge of the perfect lattice on its place, the effect of screening being taken into consideration.

The deficiency and the excess type of distortions cannot decay by themselves. They can disintegrate in separate Shottky vacancies or interstitial atoms by diffusion, but this process will probably not affect the resistance of the metal to any great extent, as the vacancies and the interstitial atoms can disappear only either by meeting each other or by diffusion to the surface of the crystals (Mott and Gurney, 1940). If a metal contains those types of distortions only, the resistance changes can be caused only by the aid of a large-scale diffusion, as the defects must travel considerable distances through the lattice before disappearing, and it is doubtful if such process can account for the observed resistance changes.

On the other hand, the combined type of distortion can diminish considerably in size (or vanish, if the excess is exactly balanced by deficiency) by the aid of *small-scale* diffusion, without appreciably moving through the lattice. It is thus probable that the combined type of distortions contributes to the resistance changes much more than the other types, especially at low temperatures.

* This table represents a review of measurements accessible to the author, but should not be taken as exhaustive.

We shall therefore deal with the decay theory of the combined type of distortions first, leaving the discussion of the recombination theory of the excess with the deficiency type to the end of this study.

## §3. DECAY THEORY OF THE COMBINED DISTORTIONS

### (*a*) *Fundamental assumptions*

In the part dealing with the general theory of the resistance changes, the outline of the process of irreversible changes of resistance was given. The changes were pictured as being caused by the decay of the distortions in the lattice. In order to develop a more detailed theory of the resistance changes, a set of assumptions concerning the properties and behaviour of the distortions is necessary. We cannot expect that these will be rigorously valid, as they are very probably over-simplified, but they will at least show the character of the problem with which we are dealing and the methods of attack useful in such a case.

Let us formulate the following set of assumptions as a basis of our further investigations:

(1) Each combined distortion has a higher energy content than the same number of atoms in an ordered infinite lattice, and it can thus decay or regroup into an ordered lattice, so losing its scattering properties for electrons, if one of its atoms, by a local random rise of temperature, reaches a certain critical value of kinetic energy $E$, *which brings the whole system over the potential-energy barrier.*

The energy $E$, which we shall call a *decay energy*, is a characteristic constant of each distortion and can vary from zero to values comparable with the height of the potential barrier for self-diffusional exchange of atoms in the lattice. Negative energies $E$ are of course possible, but such distortions will decay spontaneously even at 0° K.

A higher energy content of the distortions is necessary, as otherwise no appreciable decay of the type observed will occur. The decay of the distortions is accompanied by the liberation of this surplus energy in the form of a local heating distinct from the local random rise of temperature. In consequence it is probable that the decay can be initiated by any of the atoms of the distortion and, once started, continues till the whole distortion reaches the lowest state of energy.

As the proportion of atoms at any moment with kinetic energy $\geqq E$ is $\exp(-E/\boldsymbol{k}T)$, where $\boldsymbol{k}$ is Boltzman's constant and $T$ is the absolute temperature, the law of decay of the distortions can be expressed by the equation

$$dN(E,t)/dt = -cN(E,t)\exp(-E/\boldsymbol{k}T), \qquad \ldots\ldots(2)$$

where $N(E,t)$ is the number of distortions in the unit of volume at the time $t$, characterized by a common value of the decay energy $E$.

The constant $c$ is given by the equation

$$c = 4fn, \qquad \ldots\ldots(3)$$

where $n$ is the number of atoms forming the distortion, or more exactly the number of atoms which can initiate the decay of the distortion, and $1/4f$ is the

average time required for the initiation of the decay, so that $f$ is the so-called *Debye's maximum frequency* of the thermal oscillations of the lattice atoms, tabulated for some metals in table 1.

It is difficult to estimate the exact value of the constant $c$ without making more detailed assumptions about the structure of the distortions. First of all, the number $n$ can vary between wide limits from unity upwards. For the combined type it can be estimated to be of the order of 10. Secondly, it is open to discussion whether the time necessary for the initiation of the decay is $1/4f$ and whether the Debye frequency of the atoms in a distortion is the same as the frequency of the atoms in the perfect lattice. Fortunately, the variation of the exponential term is much more pronounced than the variation of $c$, and a ten-fold error in $c$ will have but little influence on the final results.

(2) Each distortion forms an independent unit in the lattice of the metal, i.e. it is not interlocked with the neighbouring distortions. This implies that we intend to deal with a metal where the distortions are in a comparatively diluted state, so that the decay of one of the distortions does not influence the decay of its neighbours, and that each distortion scatters conducting electrons independently, i.e. the law of the additivity of resistance holds. Naturally it does not hold in the case of abnormal metals.

(3) All distortions in a given specimen can be ordered according to one of their characteristic quantities, for example their decay energy $E$, so that in every small interval between the values of $E$ and $E+\Delta E$ there will be $N(E)\Delta E$ distortions. Let us assume further that all the other characteristic quantities of the distortions, at least in the sense of their averaged values, can be represented as functions of this parameter.

(4) The contribution of $N \equiv N(E)$ distortions to the total specific resistance of the metal is $r(E)N(E)$, where $r(E)$ is the contribution to the specific resistance due to the average concentration of one distortion per unit volume, and $N(E)$ is the number of distortions present in unit volume of the metal, both supposed to be functions of the decay energy $E$.

If distortions of varying decay energy $E$ are present, we can write an integral instead of the summation of individual resistances, and use $E$ as a parameter. We thus obtain

$$R_i = \int_{-\infty}^{+\infty} r(E)N(E,t)dE, \qquad \ldots\ldots (4)$$

in which $t$ is the time. This is a fundamental equation for the resistance changes.

(5) The characteristic function $N_0(E) \equiv N(E,0)$, which represents the original distribution of distortions ordered according to their decay energies $E$, for the time $t=0$ (the time of the preparation of the specimen or of the beginning of the experiment), is of special interest. It is a function only of the material, and if it is once known the whole history of the specimen can be deduced by the application of equation (2). On the other hand, from the known history of the specimen, the function $N_0(E)$ can be deduced, and this will be our real aim.

### (*b*) *Determination of the characteristic function*

There are two ways of attacking the problem.

(1) Method of *ageing*. The resistance is measured at a constant temperature and registered as a function of the time $t$.

(2) Method of *tempering*. The resistance is measured at a uniformly rising temperature at a rate of $a$ degrees per second, i.e. the temperature $T$ of the specimen at the time $t$ is given by $T=at$, and the resistance is registered either as a function of the time $t$ or as a function of the temperature $T$, the latter case being more convenient from the experimental point of view.

### (*c*) *Method of ageing*

In this method the temperature $T$ is constant, and we can write equation (2) in the following form:

$$dN/N=-c\exp(-E/\boldsymbol{k}T)\,dt \qquad \ldots\ldots(5)$$

and integrate, keeping $E$ and $T$ constant:

$$\log N/N_0=-ct\exp(-E/\boldsymbol{k}T). \qquad \ldots\ldots(6)$$

If we use the notation

$$c\exp(-E/\boldsymbol{k}T)=1/t_0(E), \qquad \ldots\ldots(7)$$

we obtain

$$N(E,t)=N_0(E)\exp(-t/t_0), \qquad \ldots\ldots(8)$$

which represents the decay of the distortions characterized by the decay energy $E$ at constant temperature.

Substituting (8) in (4), we obtain

$$R_i(t)=\int_{-\infty}^{\infty} r(E)N_0(E)\exp(-t/t_0)\,dE=\int_{-\infty}^{\infty} F_0(E)\phi(t)\,dE, \qquad \ldots\ldots(9)$$

the notation

$$r(E)N_0(E)=F_0(E), \quad \exp(-t/t_0)=\phi(t) \qquad \ldots\ldots(10)$$

being used.

The function $F_0(E)$ is also a characteristic function of the specimen, and as we do not know the function $r(E)$, we cannot separate the function $N_0(E)$ from the product, and we shall thus content ourselves by determining $F_0(E)$ only. This function, however, depends on the material and on the mode of preparation of the specimen in a similar manner to $N_0(E)$ (as $r(E)$ most probably varies but little).

Let us consider first the simple case when all distortions have the same decay energy $E=E^*$. Then (9) becomes

$$R_i(t)=F_0^*\exp(-t/t_0), \qquad \ldots\ldots(11)$$

and the resistance will fall exponentially from the initial value of $F_0^*$ to zero. It is thus easy to verify from a series of measurements if all distortions have the same decay energy by seeing if the equation (11) is fulfilled.

This can be done by plotting $\log R_i$ against the time $t$, or (if the subtraction of $R_T$ from $R$ is troublesome) we can differentiate (11), and since

$$\log(-dR/dt)=-t/t_0+\log(F_0^*/t_0), \qquad \ldots\ldots(12)$$

plot $\log(-dR/dt)$ against $t$. In either case, if we obtain a straight line, the validity of (11) is proved, and we can conclude that all the distortions have the same decay energy.

This method was applied to the measurements available (Vand, 1936), but no straight line was found. That means that in the specimens investigated, distortions of widely different energies $E$ are present.

Before going further, we shall examine more closely the function $\phi = \exp(-t/t_0)$, which appears first in equation (8). This is represented diagrammatically in figure 3 as a function of $E$, the time $t$ being kept constant. It can be easily seen that for $E=-\infty$, $\phi=0$, for $E=+\infty$, $\phi=1$, and for all other finite values of $E$ $0<\phi<1$. It can be easily shown that $\phi$ has an inflexion point for $E=E_0$, given by

$$E_0 = \boldsymbol{k}T\log ct, \qquad \ldots\ldots(13)$$

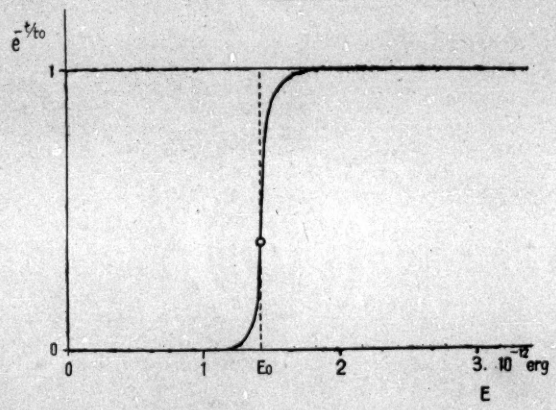


Figure 3. Function $\phi = e^{-t/t_0}$ for $T=300°$ K., $t=20$ sec.

and that at the inflexion point $\phi=0{\cdot}368$ and

$$d\phi/dE = 0{\cdot}368/\boldsymbol{k}T. \qquad \ldots\ldots(14)$$

In the laboratory conditions, for example for $T=300°$ K., $t=20$ sec., $c=10^{14}$ sec$^{-1}$, we obtain for the inflexion point $E_0 = 145 \cdot 10^{-14}$ erg and $d\phi/dE = 9 \cdot 10^{12}$ erg$^{-1}$, which are the values used in figure 3.

From the very steep slope of the curve at the inflexion point it is apparent that only a small error will be committed if we replace the function $\exp(-t/t_0)$ in the integral (9) by a broken line of value 0 for $E<E_0$, joining a vertical line from 0 to 1 for $E=E_0$ and continuing again horizontally with value 1 for $E>E_0$.

The integral (9) can then be written simply as

$$R_l = \int_{E_0(t)}^{\infty} F_0(E)\,dE. \qquad \ldots\ldots(15)$$

If we differentiate the integral (15) with regard to $t$, we obtain

$$dR_i/dt = R_i' = -F_0(E_0) \,.\, \boldsymbol{k}T/t, \qquad \ldots\ldots(16)$$

and thus obtain the equation for evaluating the fundamental function $F_0$ from the observed resistance changes:

$$F_0(E_0) = -R_i' t/\boldsymbol{k}T. \qquad \ldots\ldots(17)$$

As we have simplified the integral (9) into (15), we have introduced a certain degree of approximation into our method. For the correct interpretation of our results let us investigate more closely what kind of error we have introduced by this simplification. We can picture the function $F_0(E)$ as forming a spectrum of values, either continuous or discontinuous, and our method as having a finite resolving power. To estimate this resolving power, let us apply the formula (17) to a case where the spectrum is known. The simplest case of such a spectrum is that where $E$ is common to all distortions, so that the spectrum is

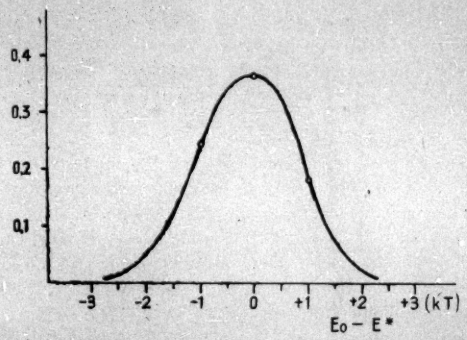


Figure 4. Blurring of a sharp line of the function $F_0(E)$ due to the approximation introduced.

composed of only one sharp line. Then the variations of the resistance are represented by the equation (11). After differentiation of this equation with regard to $t$ we obtain

$$R' = -F_0^* \frac{\exp(-t/t_0)}{t_0}, \qquad \ldots\ldots(18)$$

and by substituting this value in (17) we obtain

$$F_0(E) = \frac{F_0^*}{\boldsymbol{k}T} \frac{t\exp(-t/t_0)}{t_0}, \qquad \ldots\ldots(19)$$

If we substitute for $t$ from (13) and for $t_0$ from (7), we obtain

$$t/t_0 = z = \exp\left[\frac{E_0 - E^*}{\boldsymbol{k}T}\right], \qquad \ldots\ldots(20)$$

and we can thus write, using $z$ for abbreviation of (20),

$$F_0 = F_0^*/\boldsymbol{k}T \,.\, z\exp(-z). \qquad \ldots\ldots(21)$$

This function has a maximum for $E_0 = E^*$, for which it attains the value of $F_0(E^*) = 0{\cdot}368\, F_0^*/\boldsymbol{k}T$.

Further, this function has two inflexion points on each side of the maximum for the two values of $z$, given by

$$z_{1,2} = \tfrac{1}{2}(3 \pm \sqrt{5}), \qquad \ldots\ldots(22)$$

or for the values of

$$E_0 - E^* = \pm 0{\cdot}963\, \boldsymbol{k}T, \qquad \ldots\ldots(23)$$

as can be easily shown by differentiating the equation (21) and putting the second derivative equal to zero. Our treatment had blurred the sharp line into a diffuse one of a bell-shaped form and of a width of the order of $\pm \boldsymbol{k}T$, and with a maximum at the exact position of the original sharp line, as is shown in figure 4. The conclusion is thus, that the resolving power of our treatment is of the order of $\boldsymbol{k}T$, which is, as can be easily shown, approximately 2·5 % of the energy $E$ in the region investigated.

### (*d*) *Method of tempering*

If the temperature $T$ is rising uniformly at a rate of $a$ degrees per second from the initial temperature $T = 0$, the temperature at any time $t$ is given by $T = at$, and substituting this value in (2), we can write

$$dN/dt = -cN \exp(-E/\boldsymbol{k}at). \qquad \ldots\ldots(24)$$

Before going further, let us consider again the simplest case, where there is only one value of $E > 0$ common to all distortions and the spectrum $F_0$ consists of one sharp line only. Then for $t = 0$ we have $dN/dt = 0$, since the temperature is zero, and the function $N$ will start as a line $N = N_0$ parallel to the time axis. This corresponds to the case of the temperature being still too low to start any appreciable decay of the distortions of positive decay energy.

For very high temperatures or for $t = \infty$, $dN/dt = -cN$, but as at high temperatures all distortions will disappear, evidently we shall have $N = 0$ and $dN/dt = 0$.

We can write (24) in the following form and integrate:

$$\int_{N_0}^{N} dN/N = -c \int_0^t \exp(-E/\boldsymbol{k}at)\, dt. \qquad \ldots\ldots(25)$$

By using the substitution

$$\boldsymbol{k}at/E = x,$$

we can write

$$\log N/N_0 = -cE/\boldsymbol{k}a \int_0^x \exp(-1/x)\, dx = -yH(x), \qquad \ldots\ldots(26)$$

using the notation

$$\int_0^x \exp(-1/x)\, dx = H(x), \quad y = \frac{cE}{\boldsymbol{k}a}. \qquad \ldots\ldots(27),\ (28)$$

We can thus write

$$N(t) = N_0 \exp[-yH(x)], \qquad \ldots\ldots(29)$$

which represents the changes in the number of distortions, and if we replace $N$ by $R_i$, also the changes of the resistance.

As the temperatures used are of the order of 100° K. and the energies to be expected are of the order of $10^{-12}$ erg, the values of $x$ are of the order of $10^{-2}$. For small values of $x$ the integral $H(x)$ is approximately given by

$$H(x) = x^2 \exp(-1/x), \qquad \ldots\ldots(30)$$

with a proportional error less than $2x$, i.e. $<2\,\%$ for $x<0{\cdot}01$. The equation (29) can thus be written for small $x$ in the form

$$\log N = \log N_0 - x^2 y \exp(-1/x). \qquad \ldots\ldots(31)$$

To assist the calculations, some of the values of the integral $H(x)$ are given in table 3.

It can be easily shown that the function $N(t)$ has an inflexion point given by

$$E/\boldsymbol{ckat}^2 = \exp(-E/\boldsymbol{kat}), \qquad \ldots\ldots(32)$$

or, if we use the notation

$$E/\boldsymbol{kat} = u, \quad ctu = cE/\boldsymbol{ka} = y, \qquad \ldots\ldots(33)$$

Table 3. Values of $H(x) = \int_0^x \exp(-1/x)\,dx$

| $x$ | $H(x)$ |
|---|---|
| 0·01 | $3{\cdot}65 \cdot 10^{-48}$ |
| 0·02 | $7{\cdot}41 \cdot 10^{-26}$ |
| 0·03 | $2{\cdot}81 \cdot 10^{-18}$ |
| 0·04 | $2{\cdot}06 \cdot 10^{-14}$ |
| 0·05 | $4{\cdot}71 \cdot 10^{-12}$ |
| 0·06 | $1{\cdot}86 \cdot 10^{-10}$ |
| 0·07 | $2{\cdot}74 \cdot 10^{-9}$ |
| 0·08 | $2{\cdot}08 \cdot 10^{-8}$ |
| 0·09 | $1{\cdot}03 \cdot 10^{-7}$ |
| 0·10 | $3{\cdot}84 \cdot 10^{-7}$ |

the condition for the inflexion point is

$$u + 2\log u = \log y \qquad \ldots\ldots(34)$$

or

$$u + \log u = \log ct. \qquad \ldots\ldots(35)$$

The first derivative at the inflexion point is

$$dN/dt = -Ncu^2/y, \qquad \ldots\ldots(36)$$

and attains a very high value, and the $N$ curve is thus very steep at this point, closely resembling the curve represented in figure 3, discussed previously.

This suggests the use of an analogous treatment of the problem by replacing the smooth curve for $N$ by a broken line first parallel to the $t$ axis and equal to $N=N_0$ for $t<t_i$, then a vertical line of $N_0>N>0$ for $t=t_i$ and then a horizontal line again of $N=0$ for $t>t_i$, where the time $t_i$ is the co-ordinate of the inflexion point.

Now we can easily solve the general case of an arbitrary fundamental function $F_0(E)$ if we replace the general integral (4) by a simplified integral,

$$R_i(t) = \int_{E_0(t)}^{\infty} r(E) N_0(E)\,dE = \int_{E_0(t)}^{\infty} F_0(E)\,dE, \qquad \ldots\ldots(37)$$

thus replacing the smooth decay curves of the individual types of distortions characterized by the decay energies $E$ by a set of broken lines.

The integral (37) has the following physical interpretation:—

For each value of the time $t$ there exists a certain decay energy $E_0(t)$, which divides all distortions into two groups. One group contains all distortions of decay energy $E<E_0$. These distortions have already disappeared, and thus do not contribute further to the resistance of the specimen. The other group contains all distortions of decay energy $E>E_0$. Those distortions are still present in the specimen in their original numbers $N_0(E)$, and the resistance $R_i(t)$ of the specimen is thus given by the integration of the contributions of the second group only. As the time progresses, the limit of integration $E_0$ increases, and thus the resistance diminishes.

Now it is easy to evaluate the fundamental function $F_0(E)$ from the observed changes of the resistance. Let us differentiate the integral (37) with regard to time:

$$dR_i/dt = -F_0(E_0)dE_0/dt. \qquad \ldots\ldots(38)$$

Table 4

| $u$ | $U$ | $\log_{10} ct$ | $\log_{10} y$ |
|---|---|---|---|
| 20 | 20·952 | 9·98 | 11·28 |
| 25 | 25·962 | 12·26 | 13·65 |
| 30 | 30·968 | 14·51 | 15·98 |
| 35 | 35·972 | 16·72 | 18·30 |
| 40 | 40·976 | 18·98 | 20·55 |
| 45 | 45·978 | 21·20 | 22·84 |
| 50 | 50·980 | 23·40 | 25·10 |

The value of $dE_0/dt$ can be obtained from (32) by differentiation:

$$\frac{dE_0}{dt} = \frac{E}{t}\frac{2+ct\exp(-u)}{1+ct\exp(-u)}, \qquad \ldots\ldots(39)$$

and by the aid of (33) we obtain

$$\frac{dE_0}{dt} = ka\frac{u(u+2)}{u+1} = ka\,U, \qquad \ldots\ldots(40)$$

where we write $U$ instead of the fraction. By substituting (40) in (38) we obtain finally

$$F_0(E) = -\frac{dR_i/dt}{ka\,U}. \qquad \ldots\ldots(41)$$

As it is more convenient to measure the resistance as a function of $T$, we can use the relation

$$dR_i/dt = dR_i/dT\,.\,dT/dt = a\,dR_i/dT, \qquad \ldots\ldots(42)$$

and write, instead of (41),

$$F_0(E) = -\frac{dR_i/dT}{k\,U}. \qquad \ldots\ldots(43)$$

For the energy $E$ we shall have, instead of (33),

$$E = ukT, \qquad \ldots\ldots(44)$$

where $u$ is given by (35).

It can be shown by a similar calculation to that used before that the resolving power of this method is again of the order of $kT$, which represents the blurring of a sharp line, as represented in figure 4.

For practical calculations table 4 may prove to be helpful.

The calculation can be carried out as follows:—

(1) The observed resistance $R(T)$ is differentiated with regard to $T$ in order to obtain $dR/dT$.

(2) The part $dR_T/dT$ is subtracted from $dR/dT$ in order to obtain $dR_i/dT$.

(3) A set of values of $ct$ is estimated from the known rate of rise of the temperature, and to them are found corresponding values of $u$ and $U$ by means of table 4. Only a few values of $ct$ are needed, as $u$ and $U$ vary slowly with $ct$.

(4) The values of $F_0(E)$ are now calculated by aid of (43), and the corresponding values of $E$ are found from (44).

### (*e*) *Comparison of the method with experimental results*

(1) *Method of ageing.* The resistance variations of Ag and Au films prepared by evaporation on a glass plate have been measured at $T = 294°$ K. by the author and published previously (Vand, 1936). This is a brief summary of the results. The metals were evaporated from a tungsten coil outgassed by pre-heating in a high vacuum maintained by a high-speed condensation pump. The supporting glass plate was carefully cleaned, first chemically and then in vacuum by a glow discharge, but not baked at high temperature. Films from 10 to 50 m$\mu$ thick were prepared in 20 to 50 sec. of actual evaporation. Particular attention was given to the stability of contacts to the measured film. After trying several methods, the best contacts were prepared by welding steel Poldi AKX plates to the edges of the glass plate, evaporating a preliminary layer of Ag over the ends, so that a gap was left for the film to be measured, and reinforcing this preliminary layer by electrolytic copper plating.

Preliminary measurements of the variation of resistance showed that the films are very sensitive to slight variations of temperature and that a provision for keeping the temperature constant is necessary. Therefore some of the specimens were placed in a Dewar flask to ensure a constant temperature over long periods of time (the effect of exposure of the films to air was measured, and it was found that it causes only a very small and constant increase of resistance). This method proved to be successful, as is best demonstrated by the particular smoothness of the curves obtained. No abrupt steps, as noticed by Mitchell (1938), were observed.

All films showed a nearly hyperbolic decrease of resistance, and approximately 350 sec. after their preparation the variations conformed within close limits to the empirical formula

$$R = R_\infty \left(1 + \frac{\tau}{t+\tau_0}\right), \qquad \ldots\ldots(45)$$

where $R_\infty$ is the asymptotic value of the resistance $R$ for the time $t \to \infty$, and $r$, $\tau_0$ are two empirical constants, $r$ varying between 23 and 515, $\tau_0$ varying between 73 sec. and 3270 sec. for different films prepared under apparently similar conditions. In the region of small $t$ the resistance was generally decreasing steeper than according to the hyperbolical formula (45), but as the measurements immediately after the film preparation were unreliable and the constancy of temperature not assured owing to the heat radiated by the spiral, no attempt was made to interpret the variations in this region.

A possibility that the resistance changes are caused by some process of (large-scale) diffusion, as between the film and the supporting material, or of a gas into or from the film, was considered, and theoretical formulae of resistance changes derived and compared with the measurements, but no agreement was found, so that the explanation of the resistance changes by any such diffusion

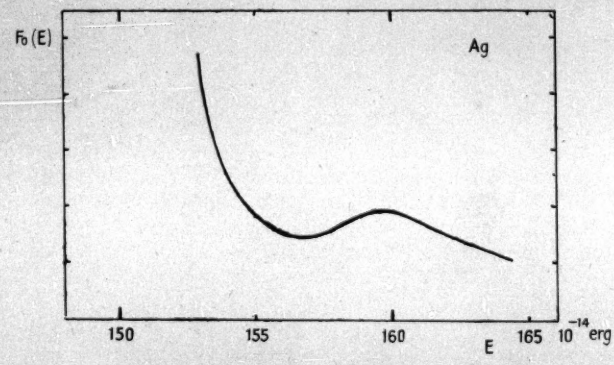


Figure 5. Characteristic function $F_0(E)$ of Ag film (specimen no. 7) obtained by the method of ageing from measurements by the author.

process (including the diffusion of the Shottky defects and interstitial atoms to the surface of the film) is to be abandoned.

A theory of the decay of lattice distortions has been developed, but a different approximate method of calculation of the function $F_0$ was then used, the solution being developed in a series and compared with experiment. A maximum of $F_0$ in the region of the decay energy $E$ between $150{\cdot}9 \,.\, 10^{-14}$ erg and $166{\cdot}3 \,.\, 10^{-14}$ erg was found in all cases.

As the method of calculation discussed in this paper is superior to the method previously used, as it gives the shape of $F_0$ not only in special cases, but quite generally, the measurements were recalculated and the functions $F_0(E)$ traced. To provide a practical example, one function $F_0(E)$ of a silver film (specimen no. 7) is represented in figure 5. If we compare this curve with the previously published results, we see that in the region of small time ($t < 350$ sec.) or corresponding energies $< 155 \,.\, 10^{-14}$ erg there seems to be a big increase of the distortions ignored in my first publication for reasons stated above, and it is

open to question if this increase is real. The curve, however, shows a maximum for $E = 160 \cdot 10^{-14}$ erg, identical with the maximum of $159{\cdot}1 \cdot 10^{-14}$ erg found previously.

This example is very instructive, as it shows clearly the limitation of the method of ageing; it only gives a very small part of the total range of the function $F_0(E)$, unless accurate measurements are extended to very large or very small values of $t$. Very long periods of observation need, however, thermostatic arrangements for keeping the temperature closely constant, and very short times are limited by the finite time of the preparation of the specimen.

It is, however, not impossible to shorten the time of preparation of the specimens considerably by a suitable technique, for example by evaporating a wire suddenly by a heavy current of a short duration generated by a condenser

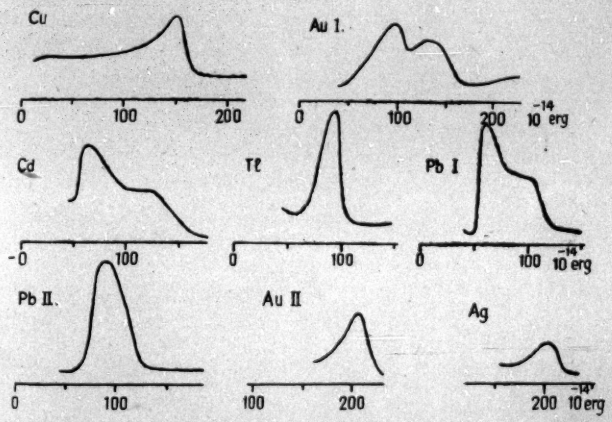


Figure 6. Characteristic functions $F_0(E)$ of some metals obtained from measurements of Suhrmann and Barth.

discharge, and so extend the range of the method, especially by the use of self-recording instruments.

(2) *Method of tempering.* Owing to war-time difficulties, the author had not the opportunity to make his own measurements at low temperatures, and the theory of tempering has been applied to measurements published by Mitchell (1938), Suhrmann and Barth (1934, 1936) and Fukuroi (1937, 1938). To provide an example, some of the typical curves $F_0(E)$ obtained from measurements of Suhrmann and Barth are reproduced in figure 6.

If we inspect the curves $F_0(E)$ we notice that they show one or more maxima, generally much broader than is the resolving power of the method of calculation. This is in accordance with the result of the method of ageing discussed before, that the changes cannot be described by a single value of the decay energy $E$. The values of the energy $E \equiv E_M$, for which $F_0(E)$ have maximal values, are of a particular interest. For this reason, those energies are reviewed in table 2

together with other characteristic constants of the specimens, measurements of which were available. The following conclusions can be drawn:—

(i) The surface state of the support has a great influence on the shape and position of the maxima of the curve $F_0(E)$. The surface of a similar or identical lattice structure to that of the metal deposited tends to lower the energy $E_M$, whereas an adsorbed gas layer on the supporting material tends to shift the energy $E_M$ to higher values.

(ii) The thickness of the film plays an important part. Very thin films show higher energy $E_M$, which decreases with the thickness of the film. This probably means that the supporting material has the greatest influence on the layers in contact with it, but gradually loses its influence on the distant layers of the film, and thicker films have probably a non-homogeneous cross-section, containing near the supporting material distortions of higher energy $E_M$ than near the free surface.

(iii) The absolute values of the energies $E_M$ are of the same order of magnitude, but smaller than the binding energies of the metals concerned. If we compare the highest values of the energies $E_M$, found in table 2, with the binding energy, found in table 1, we obtain approximately the same ratio as that between the melting and boiling temperatures of the same metal. We can thus write (see table 1)

$$E_M \leqq E_{\text{bind.}} \cdot \frac{T_{\text{melt.}}}{T_{\text{boil.}}}.$$

Such a relation is not surprising, as the metals with relatively low melting point are expected to have lower potential barriers in their lattice than metals of comparatively high melting point and of the same binding energy.

In general, it can be said that the decay theory leads to reasonable results, consistent with our conceptions of the orders of magnitude of the energies to be expected. It remains to find whether other processes lead to similar agreement, and we shall thus turn our attention to the recombination theory of the interstitial atoms and lattice vacancies.

## §4. RECOMBINATION THEORY OF THE INTERSTITIAL ATOMS AND LATTICE VACANCIES

The recombination theory leads to a considerable simplification of treatment, as a movement through the lattice over considerable distances is necessary. The distortions cannot in consequence have a complicated structure, as they would soon break down by diffusion into their separate constituents, i.e. simple interstitial atoms and lattice vacancies. We thus deal with only two types of distortions of a uniform structure, scattering power and rate of diffusion. These can only decay either by meeting each other, or by reaching the surface of the metal.

To move from one lattice position to the other, the interstitial atom must reach a certain transition energy $E_1$, to bring it over the barrier of potential energy. In the case of the lattice vacancies, one of the atoms forming the walls of the cavity must similarly reach a certain energy $E_2$ to move through the cavity, settle at the opposite side and so cause a movement of the cavity in the opposite

direction. There are in general $n_2$ of such atoms, which can cause the vacancy to move; for a simple cubic crystal $n_2=6$.

There is at any moment a fraction $\exp(-E/\boldsymbol{k}T)$ atoms with energy $\geqq E$, and during a unit of time there will be $N.4fn\exp(-E/\boldsymbol{k}T)$ distortions, which will move to the next position, and their average speed will be

$$v=4fnD\exp(-E/\boldsymbol{k}T), \qquad \ldots\ldots(46)$$

where $D$ is the distance between their successive positions (in a simple cubic crystal equal to the spacing of the lattice atoms). The volume $dV$ swept during the movement of the distortion in the time $dt$ is, according to the elementary theory,

$$dV=Sv\,dt=4fnDS\exp(-E/\boldsymbol{k}T)\,dt, \qquad \ldots\ldots(47)$$

where $S$ is the effective cross-section of the distortion, of the order of $D^2$.

The formula (47) is capable of many refinements, as, for example, by considering that there is a certain probability that the distortion may move backwards, and so the volume swept will be actually smaller than that given by the formula (47). We shall not consider here such refinements in detail, and we shall suppose that they will result in a constant factor (of the order of unity), which we can include together with the factor for $S$ in a single constant $b$ and write

$$dV=4bfnD^3\exp(-E/\boldsymbol{k}T)\,dt=C\exp(-E/\boldsymbol{k}T)\,dt. \qquad \ldots\ldots(48)$$

There will be a similar uncertainty about the constant $C$, as discussed before, the frequency $f$ probably differing considerably in the neighbourhood of the distortions.

As the energies $E_1$ and $E_2$ of the interstitial atoms and of the vacancies will differ by a certain amount, their speeds of movement will differ considerably, so that we can consider only the distortions of smaller transition energy $E$ as moving, and the others as stationary. As we do not know beforehand which kind of distortion is more mobile, we shall designate all quantities relating to the mobile distortions by the index $m$, and to the stationary distortions by the index $s$. Thus $E_m<E_s$.

If there are $N_m$ distortions of the mobile type and $N_s$ of the stationary type in the unit volume, then there will be a decay of both of these distortions, given by

$$dN_m=dN_s=-N_mN_sC_m\exp(-E_m/\boldsymbol{k}T)\,dt. \qquad \ldots\ldots(49)$$

If $r_m$ is the additional resistance caused by the concentration of one mobile distortion in the unit volume, and $r_s$ the resistance caused by the stationary distortion, then the change of the resistance is

$$dR_i=r_m dN_m+r_s dN_s, \qquad \ldots\ldots(50)$$

and if we write $2r$ for

$$r_m+r_s, \qquad \ldots\ldots(51)$$

where $r$ is the average resistance of one distortion, we obtain

$$dR_i=-2rN_mN_sC_m\exp(-E_m/\boldsymbol{k}T)\,dt, \qquad \ldots\ldots(52)$$

which is the fundamental equation for the resistance changes due to the recombination of the interstitial atoms and the lattice vacancies.

Let us consider a limiting case first, when there is a great excess of one type of distortion over the other. If, for example, $N_m \gg N_s$, we can write

$$dN_s/N_s = -N_m C_m \exp(-E_m/kT)\,dt, \qquad \ldots\ldots(53)$$

which is exactly the same type of equation as equation (2), discussed before, and all results will be valid, the constant $c$ in the equation (2) being replaced by $N_m C_m$. Similarly in the case of $N_s \gg N_m$ we shall obtain a similar result, the constant $c$ being replaced in this case by $N_s C_m$. By applying the method of calculation already discussed we shall arrive thus at a spectrum $F_0(E)$ composed of one sharp line for $E_m$ only.

But this, as we have seen, has not been confirmed experimentally. Let us then consider a case where both types of distortions are present in equal numbers. Then $N_m = N_s$, and we can write the equation (49) in the form

$$dN/N^2 = -2C_m \exp(-E_m/kT)\,dt, \qquad \ldots\ldots(54)$$

where $N = N_m + N_s$ is the total number of distortions. This equation is different from (2), and thus needs a separate discussion. Consequently we shall consider the two simple cases, of ageing and of tempering, separately.

In the case of ageing, the temperature $T$ is constant, and we can integrate the equation (54), obtaining

$$1/N = 1/N_0 + 2C_m \exp(-E_m/kT).t, \qquad \ldots\ldots(55)$$

and for changes of the resistance

$$R_i(t) = rN = \frac{r}{1/N_0 + 2C_m \exp(E_m/kT).t}, \qquad \ldots\ldots(56)$$

or for the total resistance

$$R = R_\infty\left(1 + \frac{\tau_0}{\tau_0 + t}\right) \qquad \ldots\ldots(57)$$

an equation identical with the empirical equation (45) discussed in my previous publication.

The interpretation of the constants is

$$R_\infty = R_T \text{ (see equation (1))},$$
$$r_0 R_\infty = r/2C_m . \exp(E_m/kT), \qquad \frac{r_0 R_\infty}{\tau_0} = rN_0,$$
$$\tau_0 = 1/2N_0 C_m . \exp(E_m/kT).$$

It is thus very tempting to interpret the observed hyperbolas, at least in the interval of the temperatures used, by the recombination theory, as on this interpretation the empirical formula valid in this interval is fully explained; but we shall not go into more detail here, as a better comparison of the theory with experiment is possible in the case of tempering.

In the case of tempering we shall, as before, assume a uniform rise of the temperature at the rate $a$ degrees per second from the initial temperature $T = 0$. We shall thus have the integral

$$\int_{N_0}^{N} dN/N^2 = -2C_m \int_0^t \exp(-E_m/kat)\,dt. \qquad \ldots\ldots(58)$$

The integral on the right side of (58) is the same as the integral already discussed in (25), and we can write the solution in the form

$$1/N = 1/N_0 + y_m H(x), \qquad \ldots\ldots(59)$$

where

$$y_m = 2C_m E_m/ka, \quad x = kat/E_m = kT/E_m. \qquad \ldots\ldots(60)$$

For the resistance changes we obtain

$$R_i(t) = \frac{1}{1/R_0 + y_m H(x)/r}. \qquad \ldots\ldots(61)$$

We shall investigate where the curve $R_i$ or the curve $N$ has an inflexion point. A calculation similar to the previous one shows that the inflexion point of the curve $N(t)$ occurs for

$$N = \tfrac{3}{4}N_0, \qquad \ldots\ldots(62)$$

or for the value of $x$ given by the equation

$$H(x) = 1/3N_0 y_m. \qquad \ldots\ldots(63)$$

The slope of the curve at the inflexion point is

$$dN/dt = -3C_m N_0/8x^2 y_m, \qquad \ldots\ldots(64)$$

or, given by a more simple expression,

$$dN/dx = -3N_0/8x^2. \qquad \ldots\ldots(65)$$

This expression permits a simple comparison of the experiment with the theory.

We can proceed as follows. Plot the resistance $R_i$ as a function of $T$, find the inflexion point and draw a tangent at this point. Then mark the points of intersection of this tangent with the $T$ axis, where $N = 0$, and with the line parallel to the axis, drawn at the height of $N = N_0$ or of $R = R_0$. If we denote the $T$ co-ordinates of these two points of intersection by $T_1$, $T_2$, and the co-ordinate of the inflexion point by $T_i$, then according to the equations (65) and (61) we have

$$E_m = -k \cdot \frac{8T_i^2}{3(T_1 - T_2)}. \qquad \ldots\ldots(66)$$

Let us compare these results with experiment. The relation (62) is the easiest to verify. The inflexion points of the resistance curves published by Mitchell are situated much lower than 3/4 of the initial resistance, so that there is poor agreement in the shapes of the curves. On the other hand, some of the curves published by Suhrmann and Barth and Fukuroi show inflexion points in the position expected.

The equation (66), however, gives energies $E_m$ far too small, lying between $7 \cdot 10^{-14}$ erg and $6 \cdot 10^{-13}$ erg, which will assign to the distortions an immense mobility and a very diluted state, which is inconsistent with the order of magnitude of the energies to be expected for diffusion and with the order of magnitude of the initial resistance. We can thus conclude that the observed resistance changes cannot be explained by a mechanism of pure recombination by diffusion.

It can be said in general that whatever is the actual mechanism of the decay of the distortions, the observed changes are far too slow to be explained by any process involving a single value of the decay or activation energy $E_m$. The recombination theory thus fails, since it involves essentially only one value of $E_m$. On the other hand, the decay theory of the combined distortions leads to consistent results, since the introduction of the whole spectrum of the decay energies $E$ is possible.

It is, however, theoretically possible to introduce a similar spectrum in the recombination theory, or to develop a more general theory, dealing with all kinds of distortions. To verify such hypotheses much more experimental research is necessary than is now available, and further work is desirable.

## § 5. CONCLUSIONS

(1) The irreversible resistance changes of metallic films prepared by the method of evaporation can be explained by a process of decay of lattice distortions. For the decay of the independent distortions of the combined type the formula

$$dN/dt = -cN \exp(-E/\boldsymbol{k}T) \qquad \ldots\ldots(2)$$

is found to hold under certain simplifying assumptions.

(2) In the case of ageing of the films, the temperature $T$ is constant, and for a group of the distortions having the same decay energy $E$, the integral of the equation (2) is

$$N = N_0 \exp(-t/t_0). \qquad \ldots\ldots(8)$$

(3) If distortions of various energies $E$ are present, the resistance changes can be represented by the integral

$$R_i = \int_{E_0(t)}^{\infty} F_0(E)\, dE, \qquad \ldots\ldots(15)$$

where $F_0(E)$ is a characteristic function of the specimen, giving the distribution of the distortions according to their decay energies $E$ as a parameter.

(4) The observed resistance changes can be used for the calculation of the function $F_0(E)$ by the aid of the formula

$$F_0 = -R_i' t/\boldsymbol{k}T. \qquad \ldots\ldots(17)$$

(5) It is found that the method of ageing has serious limitations, in that the range of energies investigated is a very narrow one unless the method is extended to very short and very long periods of time. The method of tempering, or heating, of the specimen from a low temperature allows for the investigation of a much wider range of energies.

(6) In the case of tempering of the films it is assumed that the temperature is rising uniformly. Then for a group of distortions having the same decay energy $E$, the integral of the equation (2) is

$$\log N/N_0 = -yH(x), \qquad \ldots\ldots(26)$$

which represents a curve with a very steep step. The observed resistance curves are much more gradual and cannot be explained as caused by distortions all having the same decay energy $E$.

(7) If there are present distortions of various decay energies $E$, the resistance changes are given again by an integral

$$R_i = \int_{E_0(t)}^{\infty} F_0 \, dE, \qquad \ldots\ldots(37)$$

where $F_0(E)$ is the same characteristic function as in (15).

(8) The function $F_0(E)$ can be calculated from the resistance changes by the aid of the equation

$$F_0(E) = -\frac{dR_i/dT}{kU}. \qquad \ldots\ldots(43)$$

The function has been calculated for several specimens, and the results are shown in figure 6 and tabulated in table 2.

(9) A possibility that the resistance changes are caused by a recombination of the interstitial atoms and lattice vacancies has been investigated, and the following equation found:

$$dR_i/dt = -2rN_mN_sC_m \exp(-E_m/kT). \qquad \ldots\ldots(52)$$

If one type of the distortions is in great excess, the equation takes the form (2). In the case of $N_m = N_s$, the form is different and is discussed.

(10) In the case of ageing, the integral has the form

$$1/N = 1/N_0 + 2C_m t \exp(-E_m/kT), \qquad \ldots\ldots(55)$$

which is a hyperbolic equation similar to that found experimentally in my previous publication.

(11) In the case of tempering,

$$1/N = 1/N_0 + y_m H(x). \qquad \ldots\ldots(59)$$

The formula is discussed, and it is found that it is inconsistent with the observed curves.

(12) It thus seems probable that at least the biggest part of the observed resistance changes are caused by the process of the decay of the combined distortions, and not by the recombination of the interstitial atoms and vacancies.

## § 6. ACKNOWLEDGEMENTS

This research was initiated at the Spectroscopical Institute of the Charles University, Prague, but owing to the circumstances could not be finished there. I should like to thank Prof. Dr. V. Dolejšek for the inspiration of the research.

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# DISCUSSION

On paper by E. G. RICHARDSON (*Proc. Phys. Soc.* **55**, 48 (1943)).

Dr. H. HEYWOOD. The turbidimeter type of instrument is increasing in importance for the industrial measurement of fine powders, an application to which Dr. Richardson's research work has largely contributed. The size analysis of powders by means of the optical properties of suspensions in liquids actually measures the surface characteristics, i.e. the percentage of the total surface represented by the various grain sizes. Other methods, such as sedimentation in liquids and the withdrawal of a sample which is subsequently dried and weighed, give the characteristics on a weight basis, and direct microscopical measurement gives the characteristics on a number basis. These various characteristics may all be related mathematically, in theory, but in practice a large error may be introduced in the transformation from one basis to another if there is the slightest error in the actual experimental data, or if the sample examined has been of too small a quantity. Thus, in converting a microscopical count to an analysis on a weight basis, the probable error will be very high for the largest particles present in the powder. The conversion from weight to surface basis, or vice versa, is not subject to quite such large errors, but careful work is essential in order to obtain accuracy over the whole range of particle sizes examined. Indeed, when the range of particle sizes to be measured is very wide, it may be necessary to combine turbidimeter measurements on the finest particles with direct sedimentation (e.g. in the Andreasen apparatus) of the coarsest particles.

The turbidimeter method, although very convenient, and likely to be used to an increasing extent, cannot yet be considered as fully perfect and without the possibility of improvement. There are three main problems involved, namely, complete dispersion of the powder, the effect of light wave-length or colour, and the effect of refractive index. The study of complete dispersion is a matter for the colloid chemist, and although no systematic research has been recorded, a considerable amount of information is accumulating on the most suitable liquids and dispersing agents for various powders. Dr. Richardson's work has shown the relationship between wave-length and particle size necessary in order that the scattering of light may be limited to negligible proportions,

but other workers in this subject have shown that certain materials, such as red and green slates, may exhibit anomalous results by this method of measurement. The difference between the refractive indices of the particles and the liquid used for the suspension may be important in some cases. For example, zircon particles, having a high refractive index, behave as opaque particles in all liquids when the light beam is parallel, but a mineral such as silica with lower refractive index is quite sensitive to the refractive index of the liquid. When the refractive index of the liquid is the same as the particles, the latter virtually disappear, and this property may be used to advantage in the size analysis of mixtures of, say, silica and a metallic ore. By choosing a suitable liquid (e.g. mixture of benzyl alcohol and aniline) the silica particles may be made to disappear and the size distribution of the ore particles obtained separately. Similar measurements, using a liquid of low refractive index, will give the combined size distribution of the two solids, and hence by calculation the size distribution of the silica particles may be obtained.

AUTHOR's reply. Dr. Heywood's suggestion to use a liquid having the same refractive index as one of the solids dispersed in it, in order to make this solid vanish, is ingenious, but may be rather limited in practice by the difficulty of matching the two media in this way. In the electromagnetic theory of light two factors intervene where a boundary between two media is involved, viz., the relative dielectric constant (determining refraction) and the relative conductivity (determining absorption). Both of these come into the extinction coefficient for a suspension of a solid in a liquid and are brought into Jobst's, and other, theories. Matching of these factors can only be done, if at all, for one value of the wave-length, and a small deviation in the latter results in a sharp change of optical properties of the suspension. This fact is made use of in the Christiansen light filter, wherein a suspension of a quartz powder in a mixture of benzene and carbon bisulphide, when illuminated by white light, transmits only that colour for which equality of refractive index exists. A moderate change of temperature of the system suffices to stop this colour (by scattering) and to transmit another colour. It appears, therefore, that in the scheme proposed by Dr. Heywood, monochromatic light and fine temperature control will have to be used, and that only moderately transparent powders can be made to do the vanishing trick.

I agree that it is desirable to gather information on suitable dispersing agents and liquids for powders in common use, and I suggest that those who have such (unpublished) information might send it to the Physical Society in order that, together with the published data, a list might be printed of such data for the benefit of those who make turbidity measurements.

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## CORRIGENDA

NEWTON, by E. N. DA C. ANDRADE, F.R.S. (This Volume, p. 129.)

P. 129. *pro re nata* (" for the born thing ") I took to be, by transference of epithet, equivalent to " born for the thing ". My friend Sir Henry Dale has pointed out to me that *pro re nata* is a term still used in medicine, with the significance " as occasion arises ", or here " as occasion arose ".

P. 145 (last line but one). I wrote " awe ", not " envy ".

E. N. DA C. A.

# OBITUARY NOTICES

## Sir JOSEPH LARMOR

By the death of Sir Joseph Larmor, Sc.D., F.R.S., on 19 May 1942, the scientific world has lost one who made a large contribution to the transition from the classical mechanics to the new physics, and one who had also served the interests of science and of the larger world as Secretary of the Royal Society and as Member of Parliament for the University of Cambridge. That his services had been widely recognized is shown by the long list of distinctions that had been conferred upon him: Hon. F.R.S.Edin., Hon. Mem. R. Irish Acad., Asiatic Soc. of Bengal, Manchester Lit. and Phil. Soc.; Hon. Foreign Member U.S. National Acad. of Sci., American Acad. of Science and Arts, Boston, American Phil. Soc. Philadelphia, Washington Acad., R. Accademia dei Lincei, Rome, Instituto di Bologna; Correspondant Institut de France (Prix Poncelet 1918); Lucasian Professor in the University of Cambridge, Fellow of St. John's College, Cambridge; D.Sc. London; Hon. D.Sc. Oxford and Dublin; LL.D. Glasgow, Aberdeen, Birmingham, St. Andrews; D.C.L. Durham.

Larmor was born at Magheragal, Co. Antrim, on 11 July 1857. From the Belfast Academical Institution he went to Queen's College, Belfast, and after graduating there to St. John's College, Cambridge. He took the Mathematical Tripos in 1880 and was Senior Wrangler and First Smith's Prizeman, J. J. Thomson being Second Wrangler. He was then elected a Fellow of the College, and proceeded to an appointment in his own country as Professor of Natural Philosophy in Queen's College, Galway. After holding this for five years he returned to Cambridge as Lecturer in Mathematics at St. John's College. He was elected a Fellow of the Royal Society in 1892. His most important work was done and published in three memoirs in the *Philosophical Transactions* of the Royal Society (1894, 1896, 1897), and collected later as an essay to which the Adams Prize in the University was awarded. The essay was published in book form under the title *Aether and Matter* in 1900. This distinguished work made him the natural successor to Sir George Gabriel Stokes in the Lucasian Chair of Mathematics in the University of Cambridge on the death of Stokes in 1903.

In 1901 he became Secretary to the Royal Society and held this office until 1912. Later the Royal Society honoured him with the award of a Royal Medal in 1915 and with its premier award, the Copley Medal, in 1921. During his service as Secretary he received, in 1909, the honour of knighthood.

In addition to the Royal Society, Larmor was an active member of the London Mathematical Society. On the Council from 1887 to 1912, he was Treasurer from 1892 to 1912 and became President in 1914. In the same year he was the recipient of the de Morgan Medal.

From 1911 to 1922 Larmor represented the University of Cambridge in Parliament. He retired from the Lucasian Professorship in 1932 and shortly after, owing to ill-health, returned to Northern Ireland, where he lived until his death.

Most of Larmor's scientific work has been collected and edited by himself in two large volumes (Cambridge University Press, 1929, pp. 679 and pp. 831).

Of the contents of the first volume he says in the preface: "About half is of electrical character, the other half being mainly General Dynamics and Thermodynamics, including the dynamical history of the Earth, Formal Optics and Geometry". General dynamics, and particularly the summing up of it in the Principle of Least Action, was for him an abiding interest. His first paper was on "Least Action as the Fundamental Formulation in Dynamics and Physics". His major work on "A dynamical theory of the luminiferous and electromagnetic medium" constantly gives the picture of the physical universe as a single unity in which the course of unfolding of the aethereal field and the movements of the electric particles constituting matter make a certain quantity called the *action* a minimum. And years later, in editing a new edition of Clerk Maxwell's book *Matter and Motion*, Larmor adds an appendix on the Principle of Least Action. The work is abstract, eschewing as far as possible the construction of models, to which men of the stamp of Lord Kelvin had been accustomed. In it Larmor was unconsciously paving the way for the twentieth-century physics in which a scheme of relations such as those of Einstein's Theory, or some presentations of quantum mechanics, is seen to be the essential part of the picture rather than an attempt to analyse it in terms of other concepts. In fact, Sir Horace Lamb once humorously said of *Aether and Matter* that it would better have been entitled *Aether and No Matter*.

Much of the work of Larmor is in striking parallel both in content and in time with that of Lorentz. He evolves the correlation between a moving and a stationary electromagnetic system which leads on directly to the Special Principle of Relativity as enunciated by Einstein in 1905. He gives the analysis of electric current in terms of moving electrons, and that of the displacement current in terms of the change in aethereal polarization, the change in polarization of atoms, and the movement of polarized atoms. He gives too the analysis of magnetism in terms of electronic orbits.

In the course of his work on the field of moving electrons, he evolves the expressions for the radiation from accelerated electrons and also the effect of a magnetic field on electronic orbits, so giving an early explanation of the Zeeman effect.

To these labours in the electromagnetic theory of matter, Larmor added a deep concern with thermodynamic principles, and was in no small measure responsible for bringing their importance to the fore. He was much interested in the work of Willard Gibbs, to whom he pays tribute in a very instructive obituary notice in the *Proceedings* of the Royal Society. In his memoir in the same journal (Series A, vol. **81**) on the life of Lord Kelvin he gives a survey of the development of thermodynamics which deserves to be preserved for its own sake. Later, Larmor again made a very substantial contribution to scientific literature by completing the editing of the works of Lord Kelvin and Sir George Stokes.

Between the old and the new physics he stands, always conscious of his debt to the past, always labouring to free science from the shackles of the past, building the foundations of the new physics, but critical of it in its enthusiasm for new paths. A deeply honest thinker, with wide interest in the world at large, never craving for publicity, but winning respect always for his judgement and his probity, he was one of the great band of those who create and deserve the title of Professor of Natural Philosophy.

E. C.

## JOHN HAMER SUTCLIFFE

J. H. SUTCLIFFE, who died on 24 October 1941 as the result of an accident, was twice elected President of the Optical Society, and was also honorary librarian for 15 years. Although his main activities were concentrated on the development of the British Optical Association, of which he was secretary soon after its foundation in 1895 up to his retirement in 1940, he was closely connected with the Optical Society over a long period of time. He was on the Executive Committee of the 1905, 1912, and 1926 Optical Conventions, to which he contributed papers on "Sources of Error in Ophthalmometry, with Suggestions for their Avoidance", and "A new Optical Method of Scale-recording for Ophthalmological Instruments". He was also a member of the General Committee.

In his early days Sutcliffe gave as much time to music as he did to science. When he was studying ophthalmology at Manchester he was also playing the double bass in the De Jong Free Trade Hall Orchestra. For many years he was associated with the Royal Italian and D'Oyley Carte Opera Companies.

As a research worker in technical optics he specialized in keratometry. His various instruments (notably the Sutcliffe one-position keratometer), trial-frame and test-charts are well known. He was also the originator of improvements in the solid downcurve bifocal.

During the Four Years' War he devoted much time to the making of artificial eyes for the Ministry of Pensions and the manufacture of aluminium-based opaque glasses, working in conjunction with the late Sir Henry Jackson. He also worked in close touch with the Army Council and Army Medical Department in connection with the supply of spectacles to the troops, being made Superintendent of the newly-formed Army Spectacle Depôt. During the two years this department was in existence, over a million pairs of spectacles and nearly two million pairs of protective goggles were supplied to the Army. For this work Sutcliffe was awarded the Membership of the Order of the British Empire and later made an Officer of the Order. After the War he was appointed Superintendent of the Optical Appliances Department (Ministry of Pensions) and held the position of honorary optical adviser to the Ministry of Pensions. In 1921 he was elected a Fellow of the Institute of Physics. He was also an Honours Fellow of the British Optical Association.

His greatest contribution was his work for the British Optical Association. It was primarily through his efforts that the standard of optical examinations was progressively raised and that, at 65 Brook Street, headquarters were established, with one of the finest optical libraries in the world and a museum unique in its scope and interest.

His interest in international affairs was intense; every year he spent much time in contact with his optical colleagues on the Continent. As President of the International Optical League he was well known to all the European optical organizations, of most of which he was an honorary member. The regard in which he was held was manifested in the award during 1937 of the Olympic Crown of Work, which was made to him as the outstanding figure of his profession in Europe.

Like all men of dynamic personality, he did not escape criticism, but no one will ever question that for the first third of this century his was one of the outstanding personalities in ophthalmic optics.

G. H. G.

## CHARLES ROBERT DARLING

BORN at Crewe in 1870, Charles Robert Darling received his scientific education at the Royal College of Science, Dublin, and soon after completing his studies he settled down in Woolwich, where he was appointed a part-time lecturer in Science at the Royal Military Academy. At the same time he also taught at the City and Guilds of London Technical College, Finsbury, and he continued both these activities till August 1926, when the College was finally compelled to close down, after which he joined the staff of the Royal Military Academy as a full-time instructor. A few years later he was appointed Assistant Professor of Science. Thus most of his time and energy were absorbed in teaching, to which he gave himself wholeheartedly and with unstinted devotion. His lectures were clear, concise and direct, and by concentrating on essentials he had the gift of making difficult subjects seem straightforward and simple to his students; but his fame as a lecturer extended beyond the limits of the class-room, as his many courses before the Royal Society of Arts testify. Darling had no patience with any humbug or evasion in presenting his subject, and this attitude of mind was characteristic of all his dealings.

He took a great delight in experimenting, and his work on surface tension affords an example of his love for inventing demonstrations of great beauty. I remember, too, how, when one of his lecture assistants noticed a curious effect with a hot wire carrying an alternating current, he seized on the observations to devise a very elegant method of showing to an audience the possible modes of vibration of a wire under tension. His early invention of a calorimeter for measuring the calorific value of fuels is another example of his experimental skill, and the apparatus still affords a simple and convenient method when an accuracy of 1 or 2 per cent suffices. But Darling's contribution to physics will be best remembered by his experiments on the thermo-electric properties of fused metals and alloys published in the *Proceedings* of the Physical Society. These difficult investigations gave results of interest and point the way to further work on these obscure problems. In his early days at Woolwich his activities were directed, in association with the late Claude Freese-Green, to cinematography, then in its infancy.

Darling's interests were not confined to science. He was an active member of the Woolwich Rotary Club and a keen golfer. He was also well read in many subjects, and he took a special interest in mountaineering, although he was unable to do much climbing himself. He served on the Councils of the Faraday Society and of the Physical Society, of which for a time he was a Vice-President, and for many years he was a member of the R.E. Board; he was a Fellow both of the Institute of Chemistry and of Physics. He was a member of the Society of Friends.

When in 1930 Darling retired from the staff of the R.M.A., after 30 years of service, he found more freedom to devote himself to golf; but it was not for long that he was able to enjoy this hobby. After a few years the long and painful illness which finally crippled him began to descend on him, and soon it was only with great difficulty that he was able to reach the golf club near his home to meet his friends. At the outbreak of the war he and his wife sought refuge from the impending rigours of life in London by retiring to the Welsh border country. When I bade him farewell, he was already so infirm in body, though still fresh in mind, that I feared there was little chance of meeting him again;

that, alas! was the last occasion on which I saw him. Three years later he was attacked by a severe cold which developed into congestion of the lungs; by then his constitution was in no way equal to resisting such an attack, and he passed away on 10 October 1942, and was laid to rest in the churchyard of the neighbouring village of Llanymynech. He leaves a widow, two sons and a daughter. W. M.

C. R. DARLING joined the staff of the Electrical Engineering Department at the City and Guilds Technical College (known now affectionately as "Finsbury") in 1899, and established a very high reputation as a teacher. His lectures on physics were a delight to all students attending them; his grasp of his subject and the clear manner of his explanation, coupled with the numerous experiments he devised to illustrate his lectures, established him as the "master" teacher.

"Finsbury", built in the year 1883 near to Old Street by the City and Guilds of London Institute, was the forerunner of Institutes for the advanced study of technology. Throughout its long career, tragically ended in 1926, its reputation, jealously guided by eminent men, such as Professors Perry, Morgan, Dalby, Coker, Armstrong, Eccles and Silvanus Thompson, remained high. Men educated at Finsbury will be found today in all parts of the world, and whenever these old students meet they refer affectionately to the mother of their career. The eminence of many men who owe their success to the start given them at Finsbury speaks highly of the basis of the teaching. The fundamental aim of Finsbury was to provide just that type of serviceable course to fit a man for a career in industry and to eliminate work of a highly academic character.

Darling will be remembered by all those who studied under him for the meticulous care with which he kept this aim of Finsbury before him. J. C.

## NICHOLAS EUMORFOPOULOS

NICHOLAS EUMORFOPOULOS belonged to his College as few men have done. Born on 9 May 1872, he entered University College, London, with an Entrance Scholarship in 1889, having been for an immediately preceding period a pupil in University College School, then housed in a wing of the College. He remained in active and regular work in the College for the rest of his life. When he died on 9 December 1942 he had thus been in continuous association with the Gower Street building and the activities therein for some sixty years.

His long service in the College changed much in character at about the middle period. In the first phase, both as a student and in postgraduate research, he displayed notable powers as a physicist, and seemed likely to attain eminence. His growing distinction was recognized by his appointment to the teaching staff in 1894 and his election to Fellowship of the College in 1900. But from 1912 onwards, having become at that date the Honorary Treasurer of the College Union Society, he devoted himself more and more, apparently by deliberate choice, to promoting the welfare of the students in the social and athletic aspects of College life. And after 1919, although still an Honorary Research Assistant, he spent nearly all his time in the manner just indicated.

It is for this generous service that he will be remembered most, at any rate by those associated with the College, to all of whom he was known affectionately by the name of "Eumo". A tribute to this aspect of his work—his true life's work as it is described—has been put on record elsewhere by the College authorities.

Here we may glance especially at his work as a physicist. Eumorfopoulos graduated in London with First Class Honours in 1892. Thereafter he began research under Carey Foster, and was awarded a research medal in 1896. He joined the staff as a regular Assistant in the Physics Department in 1894 and continued in this capacity until 1919, during most of this period being in charge of the Advanced Laboratory. He did not give lectures frequently or regularly; his preference was for individual instruction and the opportunities it gave for emphasizing the importance of precision in measurement. The writer, who both studied under him and was trained by him to demonstrate, pays grateful tribute to his powers as a teacher and to his kindly qualities as a man.

Under the influence of Callendar, who succeeded Carey Foster in 1896 and continued for a few years as Professor of Physics, Eumorfopoulos began experimental research in Heat, and his chief work was his measurement of the boiling point of sulphur, using the constant-pressure gas thermometer which bears Callendar's name. The research was a protracted one. For, like all that Eumorfopoulos undertook, this task was characterized by his unswerving determination that the results should be of the highest possible precision. The nitrogen used in the thermometer must be prepared pure; so also must be the mercury employed. Any apparatus in any way suspect was rejected and replaced, and mutual consistency of numerous observations was a fundamental requirement. The most severe critic of the work was the experimenter himself, and he would not publish until he was sure.

A less well known piece of work carried out by Eumorfopoulos was his repetition of the Joule-Thomson porous-plug experiment, initiated at the instance of J. Rose Innes, who provided the funds for the necessary apparatus. Some results were eventually published, but there is reason to believe that Eumorfopoulos was never fully satisfied with them.

Although he ceased active work in physical research many years ago, his interests having been diverted in the manner described, it is of interest to note that as recently as 1941 he must have read parts at least of the Society's *Reports on Progress in Physics* published in that year; for he entered a protest against Ferguson's statement therein that King's College was the first in London to institute instruction in practical physics. Loyal again to his *alma mater*, and quite possibly with historical truth, he claimed this distinction for Carey Foster and University College.

A. O. R.

# REVIEWS OF BOOKS

*A Treatise on Physical Chemistry*, edited by HUGH S. TAYLOR and SAMUEL GLASSTONE. Vol. 1, *Atomistics and Thermodyamics*. Pp. vi+679. (New York: Van Nostrand Co.; London: Macmillan and Co., Ltd., 1942.) 42*s*.

This is called the third edition of the *Physical Chemistry* which was first published in two volumes by H. S. Taylor and a number of collaborators in 1925. At that time it was thoroughly up to date, and as comprehensive as could be asked of a textbook. The new edition, however, will far surpass it in this latter quality. The two volumes are to become five, and a second editor, himself a writer of a highly valued textbook, is associated with Professor Taylor.

The first volume, on *Atomistics and Thermodynamics*, will apparently share with the second volume the task of presenting the more physical side of the subject, leaving the last three to present the more chemical aspects. It has only four chapters, but what chapters they are! The senior editor contributes the first, on *the atomic concept of matter*. Wave mechanics is first mentioned on page 23, and thereafter it dominates the book, in the sense that no phenomenon is explained on a classical or old-quantum-theory basis if wave mechanics has a fuller explanation to offer. Isotopes naturally loom large in this first chapter, and there is quite a good description of the experimental methods used for separating them. A valuable table, occupying 14 pages, lists the "artificial" radioactive atomic species, giving their half-lives and the energies of their radiations.

The second chapter, by Dushman, is 219 pages long, and might well have been a textbook on its own. It deals with quantum, i.e. wave-mechanical, theory of atomic spectra and atomic structure, and is rather more difficult reading than the others; it would not, for example, be suitable as a textbook for honours candidates, though it would be of great value to young (as well as to old) research workers confronted with a need for a more detailed knowledge of some part of this subject. A mere list of its contents would be dull and lengthy, but we may note that hyperfine structure, poly-electronic atoms, magnetic properties and nuclear spin are all treated, as well as the quantum theory of valency, where a detailed account of the theory of the hydrogen molecule is given.

H. S. Taylor's chapter on classical thermodynamics offers less scope for novelty, but in treatment it is both sound and clear. The notation is naturally that of the Lewis school, which, despite an initial reluctance to see $F$ used otherwise than for Helmholtz's free energy, has been accepted by so many in this country, as well as in the U.S.A., that it would now be absurd not to adopt it. The statistical basis of the second law is briefly discussed, with an account of the structure and use of the partition function, but this subject is taken up in greater detail in Chapter IV, on *the third law of thermodynamics and statistical mechanics*, written by J. G. Aston. Here the treatment is based on modern ideas, practically all trace of the historical development having been eliminated, with considerable gain in clarity. The Fermi-Dirac and Bose-Einstein statistics are introduced in a way to give logical satisfaction to the student.

In the course of this chapter, the adiabatic demagnetization method of cooling is described, and it is satisfactory to note that the difference between the "Curie" temperature scale and the thermodynamic scale is emphasized, and their relation illustrated in a graph.

It is distressing to reflect that a recommendation to all physicists to get hold of this treatise is tantamount to asking for more risk for our seamen; but, at any rate, those who can obtain a copy that is already in this country should certainly do so. J. H. A.

*Thermionic Tubes at very High Frequencies*, by A. F. HARVEY. Pp. viii + 235. (London: Chapman and Hall, Ltd., 1943.) 18*s*.

Considering time as measured by the sequence of events we can say truthfully, if tritely, that Dr. Harvey's book supplies a long-felt want. In its extensive though not exhaustive bibliography, conveniently classified into author-alphabetical lists at the ends of the

chapters, there are over 500 references, of which more than 90 % relate to work published within the last ten years. The book will be of great service to workers in the ultra-high-frequency field, who have, it must be admitted, a most formidable task in following the progress of the different branches of their subject ; it will also be welcomed by many other readers, for thermionic tubes working at very high frequencies have a rapidly increasing number of practical applications. A few diverse examples of these are in radio communication, television, medicine and the blind landing of aeroplanes.

The book has an interesting Foreword by Dr. E. B. Moullin: Of the six chapters, the first is concerned mainly with the properties of the more familiar types of thermionic tube in rectifier, amplifier and oscillator circuits, and serves as an introduction to the main theme, which opens in Chapter II where the influence of the frequency of operation on the behaviour of these tubes is discussed, and an account is included of methods of measurement of tube constants at high frequencies. Descriptions, with photographs, are given of some newer types of tube, designed to minimize unwanted effects due to electron inertia and to the capacity and inductance of electrodes and leads.

Chapter III, which is short, deals with retarding-field generators and includes experimental details of the work of Ratcliffe and Kownacki on a tube using positive ions instead of electrons as the travelling particles. The various theories which have been put forward to explain the production of oscillations by the retarding-field triode and the diode are mentioned, but are not critically discussed.

Chapters IV and V, which occupy rather less than one-half of the book, are concerned with the statical, low-frequency and high-frequency properties of the magnetron ; full details are given of the author's own work in this field. There are 171 references to published work in the lists given at the end of these two chapters.

In Chapter VI the construction and method of working of the Klystron and allied tubes are described—and the necessity of designing tube and circuit as one unit for ultra-high-frequency work becomes apparent. The author is thus justified in giving an account of electromagnetic wave guides and horn radiators, at first sight not within the purview of the book as judged from its title.

Useful reference data are included in each chapter, and although the practical point of view predominates, a good deal of theory is given in mathematical form ; the last equation is numbered 144.

As the above account suggests, the book covers a wide field, and since it is a slim volume, it is perhaps to be expected that it will be read more easily by the specialist than by the general reader ; there is an occasional lack of clearness in presentation of the subject matter and of elegance in phraseology ; for example, " Telephony etc. could be received at ten times the distance without the tube " can be read in a sense exactly opposite to that intended by the author.

The production is good ; diagrams and photographs are numerous, and, like the type, excellent ; the few misstatements and misprints will no doubt be corrected in a second edition for which it appears likely that there will be an early demand. W. A. L.

*Optics—The Technique of Definition*, by Arthur Cox, B.A., B.Sc. Pp. 336. (London: The Focal Press, 1943.) 15*s*.

The title and an examination of the table of contents of this little volume give promise that, at last, this very much neglected field of Photographic Optics has received adequate attention. A first glance through the book also gives this impression, for there are copious diagrams and tables of valuable information. On more careful reading, however, there is a feeling of frustration and inadequacy combined with impatience over the lack of cogency and appreciation of the whole subject.

The book is an attempt to explain in simple, completely non-mathematical language the principles underlying the design, construction and performance of the photographic lens. It fails in this attempt to a very large extent through poor arrangement of the subject and careless explanation of fundamental ideas. It would almost appear, from a reading of the book, that the author himself was not quite sure of his subject or of photography, and that he could with advantage have perused some of the standard and well-known books on the subject.

There are several mistakes, the most misleading one being the statement on pages 54 and 57 that the distance between the nodal points of a lens is equal to half the focal length. Surely it would also have been better to explain effective aperture when enlarging or photographing near objects in terms of the extension or distance between the lens and the plate. Finally, there appears to be some peculiar notion that the effective aperture of a telephoto F/4·5 lens of two magnifications is F/2·25 !

The general arrangement of the book would have been much improved if the Depth of Focus Tables and the lists of lenses had been collected in an appendix at the end of the volume instead of scattered throughout the text, and a complete rearrangement of the chapters, in a more logical sequence, would no doubt have been an improvement.

Undoubtedly the book will find its way into the hands of a number of photographers, and it may even prove of some interest and value to a class of camera users, but it is to be regretted that a less pretentious title was not chosen. J. H. D.

*Temperature Control*, by A. J. ANSLEY. Pp. 127, with 81 figures and 14 tables. (London: Chapman and Hall, Ltd.) 13*s*. 6*d*.

The subject of temperature control is one of prime importance in many industrial and laboratory operations, ranging from the control of the temperature of large furnaces of molten steel to the maintenance of small enclosures at accurately steady temperatures for use in laboratory determinations of physical constants of materials. The literature concerning the construction and operation of thermostats for maintaining fixed or predetermined variable temperatures is scattered over a large number of different journals and catalogues. The author in this book has attempted to collect together many of the control methods used, and to eliminate those of doubtful value, but as indicated in the preface of this book, the information given must be regarded more as a guide than a complete solution for every control problem, since so many variable factors are involved.

The methods of temperature control in general use come under two general types : (i) direct expansion controls such as gas, liquid and solid expansion controls, and (ii) electrical controls such as resistance, photo-electric and thermo-electric controls ; these types are all described and illustrated in this book, with many circuit diagrams, line drawings and photographs of various commercial control units that are available. It is difficult in a book of this size to enter deeply into detail concerning the various types of controls, their limitations and defects, therefore a good bibliography is essential, and the author would have added considerably to the usefulness of this book if more extensive references to recent work on contacts, bimetallic strips, radiation thermopiles, etc., were added. Since a temperature control unit as a whole cannot be more stable in operation than its component parts, more practical information concerning the correct positioning of the control elements, of fans, etc., and methods of eliminating errors in the measurement of the temperature differential of the control might have been given.

This book does, however, partly fill the great need for a book on this extensive subject, and will prove a useful guide to those faced with a temperature-control problem as to the methods available. M. J. H.

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## RECENT REPORTS AND CATALOGUES

*Ilford Products for Industrial Applications of X Rays and Gamma Rays.* Pp. 16. ILFORD, LTD., Ilford, London.